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AUGUST, 1930

THE DIFFUSION OF STOCK OWNERSHIP IN THE UNITED STATES

SUMMARY

Growth in number of book stockholders in recent years, 561. — More rapid growth in immediate post-war period, 566. — Customer and employee sales important only after 1920, 567. — Examination of income-tax data for possible shift in ownership, 570. — Large shift in ownership from rich to less rich apparent between 1916 and 1921, none thereafter, 574. — Check on validity of figures by examination of methods of manipulation, 575; by examination of income-tax data, particularly for tax evasion, 576; by discussion of possible explanations of shift, 585; by the evidence of growth in number of book stockholders, 591. — Conclusion, 591. — Statistical Appendix, Tables I–VIII, 593.

THE United States has been called "a nation of stockholders." [1] Exaggerated tho this statement is, it reflects the generally recognized fact that a great increase in the number of stockholders has occurred in this country in recent years.[2] It is the purpose of the

1. John H. Sears, The New Place of the Stockholder (New York, 1929), p. 35.

2. Joseph S. McCoy, "The United States Legion of Capitalists," American Bankers Association Journal, vol. xix, no. 8 (February, 1927), pp. 559–560 and 626–628; "Sources of Prosperity," American Bankers Association Journal, vol. xx, no. 7 (January, 1930), pp. 643–644, 702–703.

National Industrial Conference Board, Employee Stock Purchase Plans in the United States, New York, 1928; The Conference Board Bulletin, October, 1927.

National Bureau of Economic Research, Recent Economic Changes in the United States, New York, 1929.

present article to examine this increase in detail and, in particular, to indicate that while there has been a persistent tendency for the number of stockholders to increase, this has not involved a continuing shift in the ownership of industry from the rich to the less well-to-do. A sudden great diffusion of ownership occurred in the brief period from 1916 to 1921, but after that time no appreciable change. By 1921, the rich owned a very much smaller proportion of all corporate stocks than they had owned in 1916; from 1921 to 1927 their proportion remained fairly constant.

Before considering the growth in the number of stockholders, it must be recognized that there are two quite different connotations to the word stockholder: first, the individual who owns stock and, second, the name of an individual on the stock record books of a corporation. If a man owns stock in ten companies he is one stockholder by the first connotation and ten by the second. This double meaning forces us to consider the growth in the number of individuals who own stock and the growth in the number of book stockholders.

Because of the scanty information about the number of individuals who own stock, we shall consider primarily the increase in book stockholders. In 1924, H. T. Warshow [3] made a study of the increase in stockholders, obtaining figures for a large number of corporations either by direct correspondence or from their published statements. From this material, brought up to date and supplemented by the present author, three sets of figures have been compiled: first, the number of stock-

Proceedings of the Academy of Political Science, vol. xi, no. 3, pp. 355–552.

H. T. Warshow, "The Distribution of Corporate Ownership in the United States," Quarterly Journal of Economics (November, 1924), xxxix, 15–38.

Sears, op. cit.

3. Op. cit.

holders of three particularly important corporations; second, a similar figure for a sample group of large companies; and finally, an index reflecting the number of book stockholders of all corporations.

The three largest corporations in the country, the American Telephone and Telegraph Co., the Pennsylvania Railroad, and the United States Steel Corporation, all show a tremendous growth in their list of stockholders (Table I, page 593, and Chart I, 1, 2, 3). Of the three, the Telephone Company shows the most persistent upward trend, increasing steadily from 7,535 in 1900 to 454,596 at the beginning of 1929. The Pennsylvania Railroad shows a growth nearly as persistent tho not of such great proportions, increasing from 28,408 in 1902 to 157,650 in 1929. The growth in the stockholder list of the Steel Corporation is more fluctuating, but has increased from 15,887 in 1901 to 110,166 in 1929.

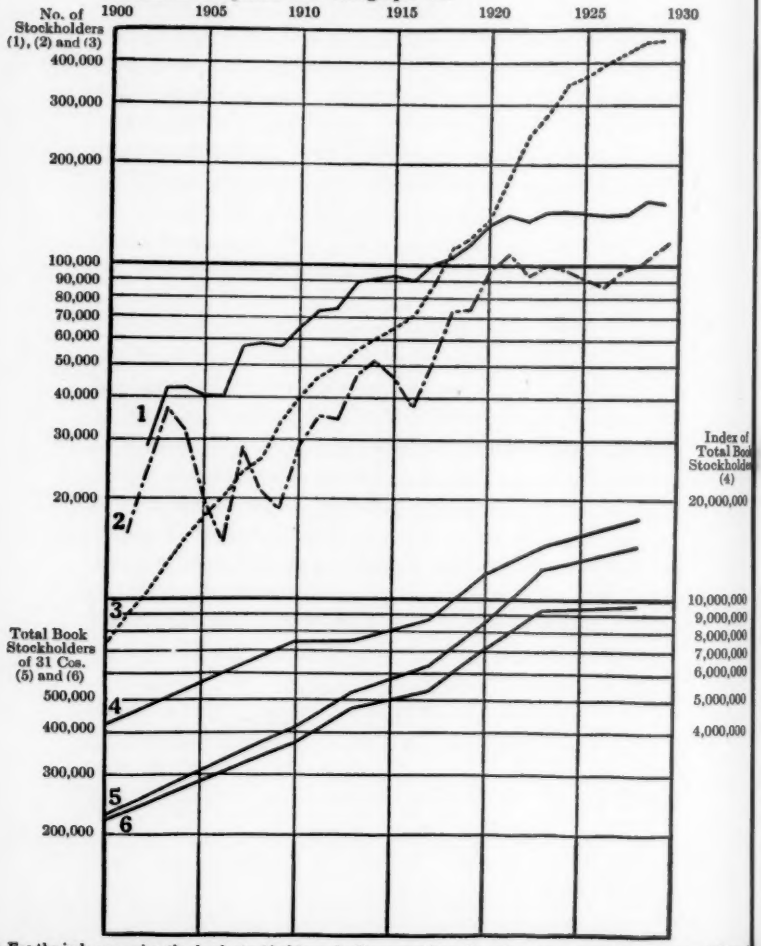
The experience of these three great companies is typical. Table II (page 594) gives the number of stockholders for thirty-one large corporations for which information was available from 1900 to 1928. The total number of book stockholders in this group (plotted on Chart I, 5) increased steadily from 226,543 in 1900 to 1,419,126 in 1928. An important proportion of the increase was due to the very great increase in the number of shareholders of the American Telephone and Telegraph Company, but even when this company is eliminated, the growth in the stockholders of the remaining companies is continuous, from 219,008 in 1900 to 964,530 in 1928 (plotted in Chart I, 6).

The indications of an increase in book stockholders shown in the preceding figures are amply sustained by material collected for a much larger number of corporations. This information, covering over 300 com-

CHART I

RATE OF GROWTH IN NUMBER OF BOOK STOCKHOLDERS

1. Pennsylvania Railroad Co.
2. United States Steel Corp.
3. American Telephone and Telegraph Co.
4. Index of Total Book Stockholders.[1]
5. Book Stockholders of thirty-one representative companies.
6. Book Stockholders of thirty-one representative companies excluding American Telephone and Telegraph Co.



[1] For the index covering the book stockholders of all corporations, there is reason to question the rapidity of growth shown between 1900 and 1910. The index is based on data for a very small number of companies in 1900, is dominated by the American Telephone and Telegraph Company which increased very rapidly in number stockholders.

panies, cannot be presented as a unit since it is incomplete for all but the thirty-one companies already cited. It can be used, however, as the basis for an index of growth. Such an index was constructed by Mr. Warshow covering the period from 1900 to 1923. It was obtained by dividing the estimated capital stock of all corporations by the average holding per stockholder in the sample companies. This method gave a figure for each year purporting to be the number of book stockholders of all corporations. Mr. Warshow made no pretence that the results were more than very rough approximations to the actual number of book stockholders. They are, however, most useful as an index of growth, particularly since he chose a method of estimating the capital stock of all corporations such as to minimize the change in number. The index has a bias which causes it to show a smaller growth throughout the whole period than probably occurred.

This index has been extended by the present author to cover 1928. The figure obtained is comparable to those of Mr. Warshow, but it is in no sense the author's estimate of the number of book stockholders in that year. It is an additional figure in an index of growth, and can be construed as only the most approximate measure of book stockholders. Above all, it must not be taken as the number of individuals who own stock.

As in the case of the large companies and the sample group of corporations, this index (Table III, page 595, and Chart I, 4) shows a persistent upward trend in the number of book stockholders, increasing from 4,400,000 in 1900 to 18,000,000 in 1928. One must conclude, therefore, that the trend toward an increase in the number of book stockholders which Mr. Warshow pointed out in 1924 has continued in the later period.

One fact stands out, however, which was not apparent

in 1924. A more detailed examination of Chart I shows that while there was an increase in book stockholders in every period since 1900, the rate of growth was much more rapid in the war and immediate post-war periods than either before or since. From 1917 to 1920 the annual rate of increase for the book stockholders of all corporations was 12 per cent and from 1920 to 1923, 6¼ per cent, while for the period before 1917 it was less than 4 per cent and after 1923 was less than 4½ per cent. For the thirty-one large corporations, exclusive of the Telephone Company, the annual growth from 1917 to 1923 was 9 per cent, while in the earlier period it was under 5½ per cent and subsequent to 1923 was less than 1½ per cent. This same difference in rate of growth is shown in the case of the three big companies. The exact period of growth is more clearly defined, since the figures are annual rather than for three-year periods. Here the more rapid growth is shown to occur between 1916 and 1921. After 1921, the steel and railroad companies showed very little growth, while the Telephone Company continued to increase its stockholders, but at a slower rate.

It appears, then, that during the war and immediately thereafter the number of book stockholders was increasing rapidly and that after that time the growth was slow.

Considerable light is thrown on this difference in rate of growth when the railroads, industrials, and public utilities are grouped separately (Table IV, page 595). The three groups show a marked difference in the basic rate of increase, but in each case the same war acceleration and subsequent slowing up appears. The information covering all the companies is not easily placed in tabular form, but that for the thirty-one companies already considered is given below, since this small group

reflects so accurately the differences which are shown when the larger body of material is examined.[4] Both the rapid growth from 1917 to 1923 and the slower growth since 1923 are clearly marked. In each period, furthermore, the growth of the railroads was slowest and that for the utilities most rapid.

For the railroads, the lack of gain in stockholders from 1923 to 1928 was undoubtedly due in large part to the efforts of single interests to acquire control of certain roads. The number of stockholders of the Erie Railroad, for example, declined from 14,495 in 1923 to 3,491 in 1928, the period during which the Van Sweringens obtained control.

The specially rapid increase in the stockholders of public utilities must be attributed in part to the very rapid growth of the larger companies in this particular industry, both by merger — a particular company acquiring new stockholders at the expense of another company — and by the addition of new capital. The customer ownership campaigns which have received so much publicity do not appear to have been an important factor before 1921. According to a compilation made by the Customer Ownership Committee of the National Electric Light Association and reproduced in Table V, (page 596,) the number of separate sales of utility stocks directly to customers amounted to less than 100,000 up to the end of 1920. Part of these were undoubtedly additional sales to the same customers, involving no increase in the names on the company's books. It is

4. For instance, in the period 1923 to 1928, twenty-two railroads, covering half the mileage in the country, showed a decline of $\frac{7}{10}$ of 1 per cent in the number of stockholders, compared with no change in the less important sample. Thirteen large public utilities, controlling nearly a quarter of all utility property, showed an increase of $12\frac{1}{2}$ per cent, compared to the 9 per cent in the small sample. A less comprehensive group of eighteen large industrials showed a growth of $2\frac{3}{4}$ per cent, as against $2\frac{1}{4}$ per cent for the lesser sample.

therefore probable that considerably less than 100,000 book stockholders were added by customer ownership campaigns during the period when stockholders were increasing most rapidly.

In the period after 1921 customer sales became very popular and must account for an appreciable share of the increase in book stockholders. By the end of 1928 a total of 1,884,148 separate sales had been made. While part of these must have involved duplication,[5] and in many cases the purchasers must have disposed of their stock before 1928, it is probable that more than a million new book stockholders were added.

There are several indications that this form of selling was primarily a post-war product. Examination of Table V (page 596) shows that the peak in new companies adopting the plan was reached in 1922, when forty-nine companies were added to those already having such plans in operation. By 1928 the number of new recruits had declined to five. This would be less significant if it were not the forerunner of a decline in the number of actual sales. These reached a peak in 1924 and subsequently decreased. The total value of shares sold annually did not begin to decline until after 1925, but by 1928 it was less than three fifths of its maximum, while the proportion of new capital raised by light and power companies through sales to customers had declined from 23 per cent in 1925 to 10 per cent in 1928.[6]

A second important factor tending to increase the number of book stockholders was the sale of securities to employees, both in the utility and other fields. Its

5. The Electrical World reports that in one case of a sale to customers by a public utility in 1929 "out of a total of 5,344 sales, 2,541 were sales to new customer owners," that is, in only 48 per cent of the sales was the customer buying for the first time and, thereby, adding to the number of book stockholders. Electrical World, vol. xcv, no. 1 (Jan. 4, 1930), p. 75.

6. Electrical World, vol. xciii, no. 1 (January 5, 1929), p. 7.

history has been in many ways parallel to that of customer sales. The increase came mainly in the period after 1921, and, while still continuing to add new stockholders, its annual addition appears to be on the decline. No figures are available for the number of sales or stockholders added each year, but the National Industrial Conference Board, in an excellent study of employee stock purchase plans, reports the year of inauguration of such plans covering "the bulk of employee-owned stock."[7] The number of plans instituted each year is given in Table VI (page 597). The real popularity of such plans began in 1919 and reached its peak in 1923, only a year after the peak for the adoption of customer ownership plans. Thereafter the decline was marked. (The small figure for 1927 probably covers only part of the year.) It is impossible to say whether this decline in new plans adopted indicates a corresponding decline in the number of new stockholders. But it is certain that the major part of such addition to the number of stockholders as was due to such plans came after 1920. By 1928 there were more than 800,000 employee stockholders.[8] The growth in book stockholders from 1921 to 1928 must therefore be due to employee sales to an important extent, both in the public utility and industrial fields.

It is a striking fact that neither customer nor employee ownership was an important factor from 1916 to 1921, when the number of book stockholders was increasing most rapidly, yet after 1921, when over a million and a half names must have been added to the books of corporations through customer and employee sales, the rate of increase in book stockholders was very

7. National Industrial Conference Board, op. cit., p. 36.

8. Ibid., p. 35. The 806,068 employee stockholders was reported as covering the bulk of such individuals, including subscribers.

slow. This still further emphasizes the difference in rate of growth in the earlier and later periods.

From all the evidence thus adduced three conclusions can be drawn: first, the trend toward an increase in number of book stockholders shown by Mr. Warshow in 1924 has continued; second, the increase was very rapid in the war and immediate post-war periods, an increase which can be accounted for only in very slight measure by customer and employee stock ownership; and, finally, the increase after 1921–23 was much slower than in the preceding period, and can be accounted for to a very considerable extent by sales to customers and to employees.

So little information is available concerning the actual number of individuals who own stocks that it is impossible to indicate their increase. It is, however, fair to assume that the large growth in book stockholders involved a considerable addition to the number of individual owners. In part, however, the added book stockholders may have simply reflected the increased popularity of diversified investment and to that extent would mean, not more individuals holding stock, but rather a greater variety held by each. On the other hand, the evidence of an increase in the number of employee and customer owners points to a considerable addition to the group of individual holders. Our further investigation will give still more evidence of such increase.

It must not be lightly assumed that, because there was a very great increase in book stockholders and, unquestionably, a large increase in the number of individuals owning stock, there was also an appreciable shift in the ownership of industry. A tremendous growth in stockholders could take place and yet affect

only a very small proportion of corporate ownership. For example, the 800,000 employee stockholders reported by the Conference Board owned stocks having a market value of approximately $1,000,000,000 in 1928,[9] or less than one per cent of all corporate stocks outstanding.[1] The total sales by Public Utilities to their customers amounted to only $1,659,000,000 [2] to the end of 1928, or less than 1½ per cent of all stocks outstanding. Thus a shift in ownership of only 2½ per cent of all stocks could have added almost two million stockholders. This would be a very great increase in stockholders, by a negligible shift in ownership. The question, therefore, is raised: Has the increase in stockholders involved an appreciable change in the proportions of corporate industry owned by the rich on the one hand, and by the less well-to-do on the other?

The answer can be found in a study of the Statistics of Income compiled by the Treasury Department from income-tax returns. Tables showing the amount of dividends reported by different income groups classified by size of income have been published for each year from 1916 to 1927. The number of individuals in each group is also reported. The figures do not cover all dividends received by individuals, since persons with small incomes are not required to file returns. In addition, the different years are not immediately comparable since the exemption limits varied from year to year. However, dividends not reported have here been estimated and allocated to the proper groups so as to

9. National Industrial Conference Board, op. cit., pp. 35 and 36. This includes stocks subscribed for but not yet fully paid for.

1. The capital stocks and surplus of all corporations in 1927 amounted to more than $130,000,000,000. Statistics of Income, 1927, p. 373.

2. See Table V.

make the figures comparable from year to year. The original data from the Statistics of Income and the estimates for the lowest groups are printed in Table VII (page 598).

Assuming that individuals received dividends in the same proportion as they owned corporate stocks, Table VII may be regarded as a fair picture of the distribution of the ownership of the stocks of all corporations. A quick examination shows a marked decrease in dividends received by the three higher income brackets from 1916 to 1921, balanced by an increase in the four lower brackets. This would suggest a shift in ownership from rich to less rich during the period. After 1921, both groups increase, but with no positive indication of change in relationship. Table VII does not therefore lend itself to a comparison of the different years. It is rather raw material from which more revealing figures can be derived. A shift in ownership becomes more apparent when the dividend receivers are divided into groups containing equal numbers of individuals, rather than individuals of equal income. It is then possible to compare the proportion of all dividends received in a particular year by, say, the individuals reporting the 100,000 largest incomes, with the proportion reported by the 100,000 largest in other years.

In reorganizing the data in Table VII into groups of equal numbers the method of interpolation employed was as follows: each column was cumulated from above and the results for each year were then plotted on single logarithmic paper, the number of individuals being measured along the logarithmic scale and the amount of dividends along the natural scale. Through the various points obtained, a smooth curve was drawn; the amount of dividends received by each particular number of in-

dividuals reporting the largest incomes was then estimated by inspection.[3]

The results of this interpolation are given in Table VIII (*a*) and (*b*) (page 600) and are converted into percentages in Table VIII (*c*). In Table VIII (*d*) the percentages have been combined into three groups: the first containing the 25,000 largest incomes, all over $35,000 in 1916; an intermediate group containing the next largest 75,000 incomes; and a group containing the remainder, individuals with less than $12,000 income in 1916. The results are plotted in Chart II.

When cast in this form the figures show two striking facts: first, a tremendous shift in ownership of corporate shares appears to have taken place in the five-year period from 1916 to 1921; second, no appreciable shift appears to have taken place in the six-year period from 1921 to 1927. In 1916 the individuals reporting the 25,000 largest incomes appear to own 57.2 per cent of all corporate shares. By 1921 this group appears to own only 36.8 per cent. In the same period the proportion owned by individuals *other* than those reporting the 100,000 largest incomes appears to have increased from 22.0 per cent to 44.0 per cent of all corporate shares. The proportion received by the intermediate group (75,000 individuals) remained fairly constant. Taken in terms of social groups this would mean that the share of corporate stocks owned by the rich and very

3. This rough method of interpolation was tested for 1926 and 1927 by means of additional data supplied in the Statistics of Income, and proved to be as accurate as the data warranted. The income-tax returns were reported by groups intermediate to those given in Table I. The dividends received by these intermediate groups were estimated from the logarithmic chart and the results compared with the actual amount of dividends reported in Statistics of Income. The maximum error found was 1.2 per cent and the mode was .4 per cent. This small error also vindicated the assumption underlying this method of interpolation, viz., that the distribution of dividends could be represented by a smooth curve.

rich, i.e. individuals with incomes of at least $35,000, declined from 58 to 36 per cent between 1917 and 1921 and remained constant thereafter, while the share owned by individuals with small to moderate incomes, i.e. under $12,000, increased from 21 to 44 per cent. At the beginning of the period half of all dividends were reported by 15,000 persons, while at the end of the period it required the combined dividends of the 75,000 largest incomes to cover half of all dividends received. This represents a shift of almost revolutionary proportions, and of great social significance.

Before examining this shift in greater detail we must raise and answer questions as to the validity of the figures themselves. Four questions present themselves. First, is the apparent shift in ownership due to the manipulation of the income-tax data, and in particular to the estimates made in the lower brackets, where complete figures were lacking? Second, even if complete figures had been supplied by the income-tax returns, could the apparent shift in ownership be due to changes in the methods of making returns in different years, rather than to an actual shift in ownership? Third, are there valid reasons why such a shift should have taken place in this five-year period and not in the period after 1921? Fourth, are there any supporting data derived from other sources which indicate such a shift in ownership?

The methods of manipulating the original data are not a serious cause of error. The one step subject to question, the method of interpolation, was checked and found to be reasonably accurate.[4] More subject to question are the methods employed in making estimates where the original data were lacking. To check the results, a second study was made similar to the first,

4. See n. 3, p. 573.

but confined to the upper income brackets for which the original data was complete except in 1916 when it was very nearly so. Such accurate original data for income groups including the half million largest incomes are supplied in Table VII. For 1916 an estimate had to be made for the dividends received by individuals with incomes between $3,000 and $5,000, but this was of small proportions. When this material was manipulated so as to show the dividends reported by the largest half million incomes, divided among the different income groups, the same shift in ownership was shown which appeared in the more comprehensive but less certainly accurate figures for all dividends. Thus, of all dividends reported in the half million largest incomes, 63.4 per cent were received by the 25,000 largest incomes in 1916 and only 46.5 per cent in 1920. In the same period the proportion received by the smallest 400,000 incomes in the group increased from 15.0 per cent to 28.0 per cent. The proportion received by this latter group is plotted in the dotted line of Chart II and shows a shift almost step by step with the shift shown where all dividends are included. It is therefore safe to say that the apparent shift in ownership indicated in the general figures was not the result of errors in the estimates made where original data were lacking. Furthermore, the estimates were made throughout with a view to minimizing the shift in ownership, particularly in the earlier years, and may for that reason be disregarded as a misleading influence.

A more serious cause of possible error lies in the changes in the reporting of incomes. These can be taken up under the following heads: failure to report dividends received; the withholding of dividends by corporations in order to save income taxes on the part of the recipient; the use of a personal holding corpora-

tion to avoid taxes; the creation of trusts for the same purpose; and the division of income among members of the family.

Since this study deals with the proportion of dividends received by different classes, a failure to report dividends received would have no effect on the results, if all classes failed to report in the same proportion. It is probable that as the machinery of tax collection improved with experience, there was less such failure. The individual with a large income was under greater pressure to avoid reporting, but at the same time he was under greater surveillance by the revenue authorities and his dividends could be more readily traced from the paying corporation. On the other hand, mere carelessness on the part of the small-income receiver, resulting in failure to report part of his dividends, would assume greater relative importance when his individual dividends were small. The only accurate data we have on the amount of failure to report is in the years 1922 and 1923, years in which the exemption limits were at their lowest and in which figures for the total dividends paid by corporations and received by corporations were published for the first time by the Treasury Department. Unfortunately the bookkeeping year for all individuals and corporations was not the same, so that a discrepancy in the amount of dividends reported by individuals and reported as paid by corporations and not received by other corporations would not necessarily indicate a failure to report on the part of individuals. However, the discrepancy in 1922 and 1923 is small. In 1922, dividends reported as received by individuals exceeded the dividends reported as paid by corporations by 28 million. In 1923 they fell short by 172 million.[5] The average for the two years indicates

5. Statistics of Income, 1922, pp. 9, 18, 19; Statistics of Income, 1923, pp. 8, 12, 13.

that of all dividends received 98 per cent were reported. Part of this failure to report must be attributed to holdings of American stocks by foreigners. According to a study of 4,367 representative corporations (reporting approximately one eighth of all corporate stocks) made by the Federal Trade Commission for 1922, 1.65 per cent of stocks were held by foreigners.[6]

Only in the early years could a failure to report have appreciably affected the figures. It is probable that in 1916 an important number of receivers of small incomes failed to file any income-tax returns, even when their income was above the exemption limits. This would cause a smaller proportion of dividends to be reported by the lower brackets than were actually received and would give a fictitious appearance of a shift in ownership, as in successive years a larger number of those previously failing actually made returns. This probability has been considered in making the estimates for 1916 and an adjustment made accordingly. An adjustment of smaller proportions was also made in subsequent years. On the whole it is probable that the failure to report dividends received has not caused an appreciable distortion of the figures.

The second possible cause of distortion is the withholding of dividends by corporations, i.e. the failure to pay out a fair proportion of earned income in the form of cash dividends. It might be expected that the large stockholders of a corporation would prevail upon the management to accumulate income instead of paying dividends upon which the recipients would incur heavy surtaxes. Where rich stockholders dominated the company, such action tended to reduce the dividends received by the large stockholders or individuals with

6. Compiled from National Wealth and Income, Federal Trade Commission, Table 82, p. 150.

large incomes more than it reduced the dividends of individuals with small incomes, giving the appearance of a shift in ownership when none had taken place. However, a study of the income and dividends of 108 large corporations, representing roughly 20 per cent of all corporate wealth, shows no change in dividend policy which could be attributed to this cause between 1910 and 1927. Only in the case of the railroads was any change noticeable. After the railroads were returned to private operation following the war, a larger proportion of each year's income was reinvested in the business and a smaller proportion was paid out in dividends. This change is easily explained by the credit difficulties of the railroads, and their serious need for capital, and cannot be construed as an effort to save the owners from income taxes. Furthermore, as the ownership of the railroads was very widespread, it is unlikely that a withholding of part of the income would cause an apparent shift in ownership. It is therefore reasonable to disregard the very slight effect which withholding of dividends in a few cases might possibly have on the apparent ownership of corporate shares.

The third possible cause of misleading appearances is the organization of personal holding companies. By placing his securities in a privately owned corporation and allowing all income to accumulate, the rich stock owner was sometimes able to avoid the surtax, since he himself would receive no dividends. If this practice had been extensive, it would distort the picture given by the figures of dividends reported by individuals, since some of the dividends formerly received by the rich would subsequently be reported as received by corporations, giving a fictitious appearance of a shift in ownership.

It is most difficult to estimate the extent of this

type of tax dodging. The income-tax law of 1916 and all subsequent acts contain a clause requiring the individual to report dividends received by such a holding company as if he, personally, had received them. A heavy penalty is placed on failure to so report them, and "the very fact that gains and profits are permitted to accumulate beyond the reasonable needs of the business shall be *prima facie* evidence of a fraudulent purpose to escape such tax." [7] The clause, however, has not been enforced and it is clear that an important amount of tax evasion has been accomplished by this means.

While a definite quantitative estimate of the distortion caused by this form of tax dodging cannot be made, an upper limit can easily be set. In 1923, the first year for which the information is available, the total dividends received by all, finance, banking, insurance, and holding companies, incorporated stockbrokers, etc., reporting a net income, amounted to only $126,700,000.[8] An important part of this must have been received by other than holding companies, particularly fire-insurance and finance companies; and some holding companies must have been active before 1916. Furthermore, some of the holding companies must have been used for other purposes than tax evasion, paying out again an important part of the dividends received. Even in the case of the companies contrived for dodging taxes, some of the dividends were probably paid out. In the case of some holding companies the dividends received may have been reported, in accordance with tax regulations, as if they had been received by the individual. It is unlikely therefore that a large part of the $126,700,000 dividends represents an effort to escape taxes. Even if rich in-

7. Federal Income Tax Law, Act of Sept. 8, 1916, 39 Stat. L. 756–777, C. 463, sec. 3.

8. Statistics of Income, 1923, p. 18.

dividuals had put stock yielding $100,000,000 of dividends into private corporations between 1916 and 1923, less than one twelfth of the shift indicated in Chart II would be accounted for. There is reason to think that even a smaller part was due to this device, probably not more than one twentieth.

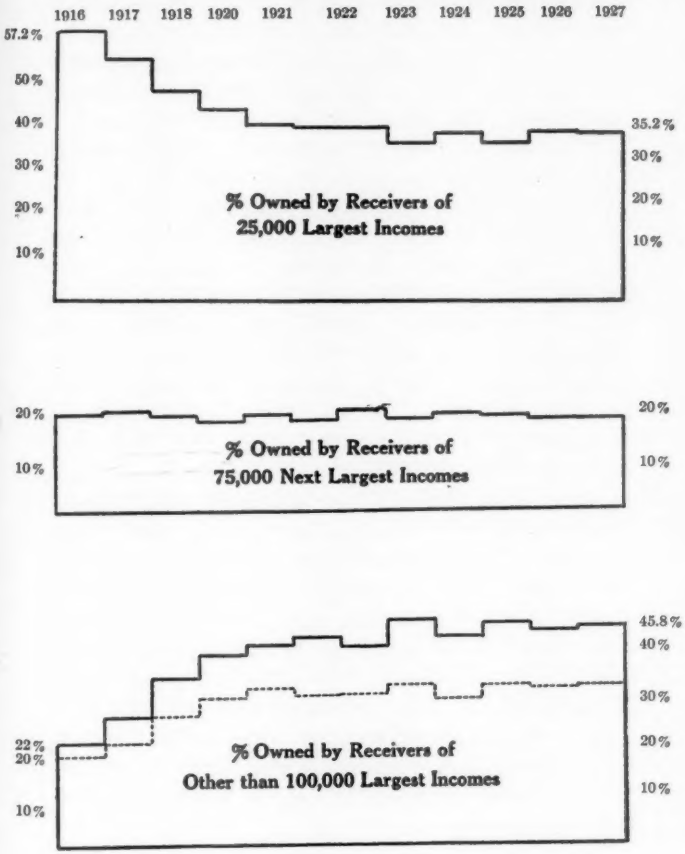
In the years subsequent to 1923, the holding corporation was more extensively employed, but to what extent it is impossible to say. It is clear, however, that it could not have caused an important distortion in the figures of the latter period.

The fourth cause of distortion was the creation of private trusts. This was also done to avoid taxes. By creating a trust in favor of a relative a man could ensure a lower rate of tax or avoid tax altogether on the income of the securities placed therein. Such a trust could often be made revocable, so that virtual control of the securities could be maintained. An approximate idea of the volume of such trusts can be obtained from the Statistics of Income. Figures for trusts alone are not given, but in 1927 the net income of all estates and trusts reporting income over $5,000 amounted to only $299,000,000.[9] The income of estates can be estimated as follows. During the year 1927, to take an example, the total estates of resident decedents with a net estate of over $100,000 had an aggregate worth of $2,260,000,000.[1] If the average estate had a life of one year, and if each estate settled was replaced by a new one which did not have a return filed during the year, the net income of all estates would amount at 5 per cent to $113,000,000. This would leave $186,000,000 as the income of trusts in 1927. Only part of this would be dividends. For the estates filed during that year, 38 per cent was

9. Statistics of Income, 1927, p. 12.
1. Statistics of Income, 1926, p. 53.

CHART II

DISTRIBUTION IN OWNERSHIP OF CORPORATE STOCKS
AS REFLECTED IN DIVIDENDS RECEIVED BY DIFFERENT INCOME GROUPS



composed of stocks of domestic corporations.[2] Applying this ratio to the income of all trusts would give dividends of $70,000,000.

While this is a very rough figure, it does indicate the relative unimportance of such trusts. Since many trusts must have been in existence before 1916, the increase for the whole period would be less than $70,000,000, and, even if it were that figure, it could account for less than one twentieth of the shift shown by Chart II.

All the factors so far considered which might lead to a fictitious appearance of a shift in ownership have on examination proved to be of relatively small significance. There remains one factor which is undoubtedly of considerable importance and whose influence it is very difficult to measure. This is the effort of the taxpayer to reduce his income taxes by distributing his wealth among members of his family, either by an actual distribution or merely for tax purposes. When the individual actually gives away property to avoid paying heavy surtaxes upon the income from it, we may regard the result as a shift in ownership, presumably involving a widening in the distribution of wealth. But when a parent, in order to avoid heavy surtaxes, gives property to his children at an earlier age than he otherwise would, there is a real question whether in the social sense a true increase in the distribution of wealth has taken place. While such a gift would cause the figures to show a shift in ownership, I am inclined to regard the shift as fictitious. It is certainly a fictitious shift when a husband gives property to his wife, so that they can save taxes by filing separate returns; or when husband and wife file a joint return in one year and subsequently file separate returns.

The cases of joint or separate returns by husband and

2. Statistics of Income, 1926, p. 52.

wife is the more simple and will be discussed first. In 1916 a couple with a taxable income of $35,000 could save $150 or approximately one fifth of their tax by dividing the income between them and filing separate returns. They could thereby avoid the surtax altogether. For individuals with larger incomes the saving would be more than proportionately greater. There was therefore considerable pressure among the rich and very rich for husband and wife to file separate returns, each accounting for part of their total income. By the Revenue Act of 1917 the surtaxes were very greatly increased, so that a couple with an income of $35,000 could save practically $1,000, or one third of their tax, if they filed separate returns. Of course not all income could be divided, and often it must have been impossible to save by filing separate returns. It is probable that in a large proportion of the cases where an appreciable saving could be made, separate returns were actually filed in 1916. Certainly this must have been true in 1917 with the greater opportunity to save.

Even tho a considerable splitting of returns did occur after 1916, it could not have caused more than a small part of the apparent shift in ownership. If we assumed that all husbands and wives in the upper brackets filed combined returns in 1916 and separate returns in 1921, it would mean that 16,000 wives [3] were included in the 25,000 largest incomes in 1916 and excluded in 1921. However, the total combined income of the 16,000 wives filing separately from their husbands and reporting the largest incomes amounted to only $314,000,000.[4] Assuming that 40 per cent of this income was received as dividends, the total dividends which might have been

3. Estimated on the basis of the proportion of all returns which involved married couples in 1921.

4. Statistics of Income, 1921, p. 51.

reported jointly in 1921 would amount to only $126,000,000. On the other hand, in 1916 the 25,000 largest returns contained approximately 3,700 cases [5] in which a husband's return did not include that of his wife. The total income reported by the 3,700 wives amounted to $156,000,000. Again assuming that 40 per cent was received as dividends, the wives filing separate returns would have reported $62,000,000 of dividends in 1916. The amounts of dividends paid out in 1916 and 1921 were approximately equal, and, therefore, if the dividends received by the 16,000 wives in 1921 amounted to only $126,000,000, the maximum dividends which could have been shifted during the period would amount to only $64,000,000 or less than one eighth of the shift in ownership which is actually shown by the figures.[6] There is ample reason to believe that the shift due to the filing of separate returns was very much less than this, probably not accounting for more than one twentieth of the total shift.

One further misleading factor remains to be considered. While the splitting of incomes between husband and wife cannot have been important, being so largely accomplished by 1916, there is no reason to believe that distribution of income among other members of the family did not continue over a considerable period. No basis is available for measuring the amount of such distribution which actually took place. However, it is apparent that it could not account for a very large part of the shift. One person out of every five could have given half of his or her estate to a relative other than wife or husband and yet only one twentieth

5. Statistics of Income, 1916, p. 24.

6. This cause of error would be more than twice as effective, for a given sum of dividends, as the two types previously considered, since it would involve not only an addition to the dividends in the upper brackets but also a reduction in those in the lower brackets.

of the shift between 1916 and 1921 would be explained. Such a gift would mean almost complete loss of legal control of the property involved. Furthermore, the recipient had to be of the immediate family, usually a son or daughter over twenty-one years of age and not already having an income sufficient to be included in the upper income brackets. Probably not more than a third of the individuals filing returns were in a position to make such a division, even if they had so desired, and the proportion actually doing so must have been even smaller.

In summary, then, only three of the factors which could give a misleading appearance of a shift in ownership seem likely to have had more than a negligible effect by themselves. Of these three factors the use of personal holding companies and of trusts may each explain as much as one twentieth of the shift in ownership, while the distribution of wealth among members of a family probably explains less than one tenth. Even when these various factors are taken in combination they do not account for more than a small part of the total shift, probably less than one fifth. The conclusion seems warranted, therefore, that the apparent shift in ownership shown by the income-tax figures reflects for the most part a real shift in the ownership of corporate shares.

This conclusion is strengthened when the possible causes of such a shift are considered. The most important single factor was the surtax itself. This, coupled with the increased income received by the lower brackets, might have been expected to cause a considerable shift in proportionate ownership. No doubt the Liberty Bond campaigns familiarized a large number of potential investors with securities and to this extent played a part. Much had been said of employee ownership and

customer ownership campaigns, but we have seen that these did not assume important proportions before 1921.

The chief influence of the surtax was to make the rich man a poor market for corporate securities. By 1919 a man with a taxable income of $100,000 was required to pay $61,000 in taxes. Even the individual with a taxable income of only $35,000 had to pay over $6,000 to the government. Plainly after paying the tax the rich and particularly the very rich did not have as large an income as formerly with which to buy corporate securities,[7] and since there is no reason to think that their living expenses decreased in proportion to the drop in income, it is evident that their expenditure on new investments must have decreased in even greater proportion. Moreover, that portion of their income which they did invest would tend to be invested in the forms in which at least a part of the tax could be avoided, i.e. tax-exempt bonds, real estate, and insurance.

At the same time the man of moderate means became a potential market for securities of all sorts. According to figures compiled by the National Bureau of Economic Research, the *per capita* realized income in terms of the 1925 dollar averaged 5 per cent[8] higher from 1916 to 1919 than in the previous three-year period. Since the income of the rich man was reduced by his taxes, the bulk of the increase in income must have gone to those less well-to-do, giving to them additional income, part of which could have been invested.

This reduction of the market for securities among the rich and the increased market among people of moderate or small means would tend to shift the proportion-

7. This is equally true if they shifted into tax-exempt securities, since the income from such securities is, as a rule, very much smaller than from taxable securities of equal risk.

8. Recent Economic Changes, ii, 763.

ate ownership of the two groups, a shift which might be of very considerable size. If each person owning corporate shares in 1916 still held them in 1921, but all new issues (which we assume to amount to 3 per cent of all issues previously outstanding) were each year purchased by individuals with incomes under $12,000, one half of the shift in ownership actually shown by the figures would be accounted for. In practice it would not be necessary that the particular new issues should be purchased by the lower group, but only that the new funds invested in corporate shares should come from this source. If the rich bought some of the new issues and sold an equivalent amount of old stocks to the lower group, the resulting shift in proportionate ownership would be the same as if the lower group bought the new issues directly. No effort has been made to measure the exact significance of this combination of factors tending to shift the proportionate ownership of industry, but it is apparent that it must have been considerable.

The surtax not only made the rich man a poor market for securities but also put him under a heavy pressure to shift into tax-exempt or partially exempt investments. If the rich as a class sold stocks, they must have been purchased by the less well-to-do, since banks and life-insurance companies could not or did not buy an appreciable volume of stocks, and foreign demand was negligible. (The investment trust or trading company was not a factor in those days.) The importance of this shifting is apparent. If there had been no increase in stocks outstanding, but the individuals with the largest 25,000 incomes had sold one fifth of their holdings to individuals with small incomes, more than one half of the shift in proportionate ownership indicated in Chart II would be accounted for. Assuming

that stocks yielded 6½ per cent in cash dividends in 1916, this would have required the selling of $4,370,000,000 of stocks between 1916 and 1921.

It has often been stated that the shifting into tax-exempt securities was not in significant amounts.[9] While this is undoubtedly true, if importance is measured in terms of loss of revenue to the government, it is probably not true in respect to a shift in ownership of corporate shares. A government report gives $9,169,000,000 as the total volume of tax-exempt securities outstanding on June 1, 1921.[1] Part of the amount was held by banks and the like, but a government actuary estimated that nearly $5,000,000,000 was in the hands of individuals.[2] If the whole of this amount had been purchased by the rich with the proceeds of stock sales almost three quarters of the shift in corporate ownership between 1916 and 1921 would be accounted for.

In addition to the wholly tax-exempt securities (chiefly state and local issues — the bulk of Liberty Bonds are subject to surtaxes) there were two types of investment whereby at least part of the surtaxes could be avoided. By purchasing life insurance, the individual could invest his money and postpone the time at which he received his income from it, thus escaping an immediate tax. If he died, the income tax would be avoided altogether since there would be none on the insurance payment to his estate. In the case of an endowment policy the odds were in his favor that the surtax rates would be lower when his time to be taxed arrived. There was thus a considerable tax-avoiding advantage in the purchase of life insurance.

9. See New Republic, vol. lvi, no. 572 (November 4, 1925), "Special Tax Section," pp. 24, 25.

1. Annual Report of the Secretary of the Treasury, on the State of the Finances, 1928, p. 567.

2. New Republic, op. cit., pp. 24, 25.

Real estate was the other type of investment by which the surtaxes could be partially avoided. To an important extent the return to the owner of certain types of real estate lies in the appreciation of his property rather than in an actual cash income. To the extent that the gain could be postponed to a time when surtax rates were likely to be lower, there was an inducement to invest in real estate rather than in corporate stocks.

One further influence is worth mentioning. To the individual paying 50 to 60 per cent of the final increment of his income to the government in taxes, the emphasis in valuing securities would shift from high return to safety of principal. Even tho he was not seeking tax-exempt investments, a safe but taxable government bond (of such there were $21,000,000,000 outstanding in June 1921 [3]) would be preferable to a high-yield but risk-bearing stock.

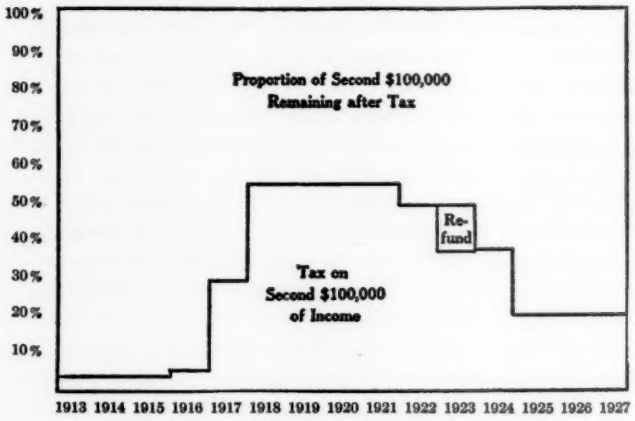
In the light of these influences, all tending to make the rich man not only a poor market for stocks but an actual seller of stocks and the man of moderate income an excellent buyer, it is not surprising to find the shift in ownership which is shown in Chart II. That the shift did not continue after 1921 is, however, surprising. The surtaxes were still heavy, tho less than in 1921, and the man of moderate means was receiving a fairly steadily increasing real income. True, the surtaxes showed a progressive drop between 1921 and 1927, as is shown by Chart III, and the expectation of such a lowering may have been sufficient to lead the rich stockholder to increase his holdings. There is no reason to believe that the figures in this later period are mis-

3. Annual Report of the Secretary of the Treasury on the State of the Finances, 1928, p. 569.

leading, since they are based not only on the reports of individuals but also on the reports of dividends paid out by corporations, and also since the various factors which would have given a false picture of shift must have run their course to such an extent before 1921 as to be negligible thereafter.

CHART III

THE WEIGHT OF SURTAXES UPON THE RICH AS MEASURED BY THE RATE OF TAX ON THE SECOND HUNDRED THOUSAND DOLLARS OF TAXABLE INCOME[1] 1913–1927



[1] Derived from Table B, Statistics of Income, 1927, pp. 35–37.

That there was a very marked shift in ownership between 1916 and 1921 and little shift subsequently is further substantiated by the figures of the growth in book stockholders which we have already examined. We saw that in the first period the number increased at a very much faster rate than in the second period, while

in the latter period the growth in the popularity of stock sales to employees and customers may well account for much of the increase in book stockholders without having caused an appreciable shift in the proportions in which stocks were held by different income groups.

To conclude: This study indicates that a great distribution of stock ownership took place during the war and immediate post-war periods, increasing both the number of stockholders and the proportion of corporate industry owned by persons of moderate means; that this widespread ownership was a legacy of the war period, much as the increase in the number of small landholdings was a legacy of the French Revolution; and that thereafter, while the number of stockholders continued to increase, this involved no appreciable shift in the proportion of corporate industry owned by the different income groups.

In addition three tentative conclusions can be drawn, which, tho more open to question, are of greater social significance. First, the great popularity of customer and employee stock-selling plans was to a considerable extent due to the drying up of the market for corporate stock among the rich and the necessity of seeking new capital among individuals of moderate means. Second, the remarkable diffusion in ownership from 1917 to 1921 was primarily the result of the heavy surtaxes of the war period, a non-recurring phenomenon, not the result of a permanent trend. And third, the surtaxes have been reduced to a point where they no longer curb the purchase of corporate stocks by the rich.

Finally, it is worth suggesting that the surtax concentrated the attention of the former owners of industry

on the possibility of retaining control without important ownership, either through the wide diffusion of stock or through various legal devices,[4] and thereby accelerated that separation of ownership and control which has become such a marked feature of our modern economy.

4. Non-voting common stock, voting trusts, pyramided holding companies, etc.

GARDINER C. MEANS

COLUMBIA UNIVERSITY

Statistical Appendix

TABLE I

Stockholders of the Three Largest American Corporations

	American Telephone and Telegraph Co.	Pennsylvania Railroad	United States Steel Corp.[7]
1929	469,801[8]	156,601[5d]	120,918[10]
1928	454,596[8]	157,650[5a]	100,784
1927	423,580	143,249[5b]	96,297
1926	399,121	142,257[5c]	86,034
1925	362,179	140,578[4]	90,576
1924	345,466	145,174	96,317
1923	281,149	144,228	99,779
1922	248,925	137,429	93,789
1921	186,342	141,699	107,439
1920	139,448	133,068	95,776
1919	120,460	117,725	74,318
1918	112,420	106,911	72,779
1917	87,000[9]	100,038	51,689
1916	71,000	90,388	37,720
1915	66,000	93,768	45,767
1914	60,000	91,571	52,785
1913	57,000	88,586	46,460
1912	50,000	75,155[3]	34,213
1911	48,000	73,165	35,011
1910	41,000	65,283	28,850
1909	37,000	56,809	18,615
1908	26,000	58,273	21,093
1907	23,000	57,226	28,435
1906	19,000	40,153[2]	14,723
1905	18,000	40,385	20,075
1904	17,000	42,230	33,395
1903	16,000	42,437	37,237
1902	12,000	28,408	24,636
1901	10,000	....	15,887
1880		13,000[1]	

[1] The Growth and Development of the Pennsylvania Railroad Co., H. W. Schotter (Philadelphia, 1927), p. 11.
[2] Ibid., p. 186.
[3] Ibid., p. 303.
[4] Ibid., p. 415.
[5] Standard Corporation Records, revised July 18, 1929, and Dec. 20, 1929.
(*a*) As of May 1, 1929.
(*b*) As of May 1, 1928.
(*c*) As of Feb. 1, 1927.
(*d*) As of Sept. 1, 1929.
[6] Standard Corporation Records, revised March 20, 1929.
[7] Wall Street Journal, October 26, 1929. (Common stock only.)
[8] Annual Report, 1929, p. 19.
[9] Bell Telephone Securities, 1929, issued by Bell Telephone Securities Co., New York, p. 10. (Derived from chart.)
[10] Standard Corporation Records, revised March 25, 1930. (Common stock only.)

TABLE II

STOCKHOLDERS OF THIRTY-ONE LARGE CORPORATIONS[1]

Name of company	Number of shareholders 1900	1910	1913	1917	1920	1923	1928
Am. Car and Foundry	7,747	9,912	10,402	9,223	13,229	16,090	17,152
Am. Locomotive	1,700	8,198	8,578	8,490	9,957	10,596	19,359
Am. Smelt. and Refin.	3,398	9,464	10,459	12,244	15,237	18,583	15,040
Am. Sugar Refin.	10,816	19,551	18,149	19,758	22,311	26,781	22,276
Du Pont Powder	809	2,050	2,697	6,592	11,624	14,141	21,248
Gen. Electric	2,900	9,486	12,271	12,950	17,338	36,008	51,882
General Asphalt	2,089	2,294	2,184	2,112	1,879	2,383	1,527
Gt. North. Iron Ore	3,762	4,419	4,685	4,855	6,747	9,313	7,456
International Paper	2,245	4,096	3,929	4,509	3,903	4,522	23,767
Proctor and Gamble	1,098	1,606	1,881	2,448	9,157	11,392	37,000
Stand. Oil of N. J.	3,832	5,847	6,104	7,351	8,074	51,070	62,317
Swift and Co.	3,400	18,000	20,000	20,000	35,000	46,000	47,000
Union Bag and Paper	1,950	2,250	2,800	1,592	1,856	2,263	1,278
United Fruit	971	6,181	7,641	9,653	11,849	20,469	26,219
United Shoe Machy.	4,500	7,400	8,366	6,547	8,762	10,935	18,051
U. S. Rubber	3,000	3,500	12,846	17,419	20,866	34,024	26,057
U. S. Steel Corp.	54,016	94,934	123,891	131,210	176,310	179,090	154,243
	108,233	209,188	256,883	276,953	374,099	493,660	551,872
Am. Tel. & Tel.	7,535	40,381	55,983	86,699	139,448	281,149	454,596
Brooklyn Union Gas	1,313	1,593	1,646	1,834	1,985	1,879	2,841
Commonwealth Edison	1,255	1,780	2,045	4,582	11,580	34,526	40,000
Western Union	9,134	12,731	12,790	20,434	23,911	26,276	26,234
	19,237	56,485	72,464	113,549	176,924	343,830	523,671
Atlantic Coast Line	702	2,278	2,727	3,404	4,422	5,162	4,212
Chesapeake and Ohio	1,145	2,268	6,281	6,103	8,111	13,010	6,885
Chicago and Northwestern	4,907	8,023	11,111	13,735	19,383	21,555	16,948
Del. Lack. and Western	1,896	1,699	1,959	2,615	3,276	6,650	7,957
Great Northern	1,690	16,298	19,540	26,716	40,195	44,523	43,741
Illinois Central	7,025	9,790	10,776	10,302	12,870	19,470	21,147
N. Y. N. H. & Hartford	9,521	17,573	26,240	25,343	25,272	24,983	27,267
Pennsylvania	51,543	65,283	88,586	100,038	133,068	144,228	157,650
Reading	6,388	5,781	6,624	8,397	9,701	11,687	9,844
Union Pacific	14,256	20,282	26,761	33,875	47,339	51,022	47,932
	99,073	149,275	200,605	230,528	303,637	342,290	343,583
	19,237	56,485	72,464	113,549	176,924	343,830	523,671
	108,233	209,188	256,883	276,953	374,099	493,660	551,872
Book Stockholders	226,543	414,948	529,952	621,030	854,660	1,179,780	1,419,126
Total Book Stockholders excl. Am. Tel. & Tel. Co.	219,008	374,567	473,969	534,331	715,212	898,631	964,530

[1] Data derived from Warshow for 1900–1923, op. cit., and from Annual Reports, Moody's Manuals, Standard Corporation Records and news clippings for 1928.

TABLE III

INDEX OF NUMBER OF BOOK STOCKHOLDERS[1] 1900–1928

Year	Total capital stock of all corporations in the United States	Average number of $100 par value shares per stockholder	Estimated number of stockholders in the United States	Annual rate of increase during preceding interval (Per cent compounded annually)
1900	$61,831,955,370	140.1	4,400,000	
1910	64,053,763,141	86.3	7,400,000	5¼
1913	65,038,309,611	87.0	7,500,000	½
1917	66,584,420,424	77.3	8,600,000	3½
1920	69,205,967,666	57.3	12,000,000	12
1923	71,479,464,925	49.7	14,400,000	6¼
1928	91,881,243,985[2]	51.0[3]	18,000,000	4½

[1] As compiled and computed by Warshow (op. cit. p. 28) for 1900–1923 and compiled by the present writer for 1928 on a comparable basis.
[2] Statistics of Income, 1927, p. 373.
[3] The wide use of no-par stock makes both the figure for total stock of all corporations and the estimate of the average shares per stockholder less reliable than in earlier years.

TABLE IV

RATE OF ANNUAL INCREASE IN NUMBER OF BOOK STOCKHOLDERS OF SELECTED CORPORATIONS[1]

	1900–1917 Per cent	1917–1923 Per cent	1923–1928 Per cent	Entire period 1900–1928 Per cent
10 Railroads	5	7	0	4⅓
17 Industrials	5½	10	2½	6
4 Public Utilities	11	20	9	12⅓
31 Companies	6	11¼	3¾	6¾

[1] Derived from Table II.

TABLE V

Stock Sales made by Public Utilities to Customers 1914–1929

Year	Number of additional companies adopting customer ownership plan [1]	Sales made* [1]	Shares of stock sold [1]	Value of sales [2]
1914	7	4,044	92,310	....
1915	3	4,357	57,130	....
1916	4	3,681	38,183	....
1917	8	8,242	82,007	....
1918	7	5,186	42,388	....
1919	12	19,872	194,021	....
1920	34	53,063	454,139	$43,000,000
1921	37	118,544	830,222	80,000,000
1922	49	156,725	1,450,707	130,000,000
1923	24	279,186	1,806,300	175,000,000
1924	23	294,467	2,478,165	254,000,000
1925	18	236,043	2,926,271	297,000,000
1926	2	248,867	2,686,187	236,000,000
1927	18	249,491	3,581,206	263,000,000
1928	5	227,961 [3]	2,081,071	181,000,000 [3]
1929	..	230,000 [3]	....	145,000,000 [3]

* In the compilation of these statistics, each separate purchase of stocks has been recorded by many of the reporting companies as being the acquisition of an additional "stockholder." There are possibilities of duplications, arising from:
(*a*) Repeat purchases of stock of the same company by the same individual.
(*b*) The purchase of stock in two or more companies by the same individual.
(*c*) In addition, the situation is further complicated by the purchase of stocks by customers from other sources than through the company's office.

[1] National Electric Light Association, Serial Report of Customer Ownership Committee 1928–29, p. 4.
[2] Electrical World, vol. xciii, no. 1, p. 27.
[3] Ibid., vol. xcv, no. 1, p. 67. Figures for 1929 are preliminary estimates based upon figures for ten months.

TABLE VI

THE INCREASE OF EMPLOYEE STOCK PURCHASE PLANS IN THE UNITED STATES

Year	No. of companies instituting stock purchase plans. [1]
1900 or earlier	5
1901–1905	13
1906–1910	14
1911	1
1912	7
1913	7
1914	6
1915	7
1916	10
1917	11
1918	8
1919	24
1920	46
1921	35
1922	17
1923	51
1924	29
1925	29
1926	13
1927	4
No information	49

[1] Compiled from appendix of National Industrial Conference Board, op. cit.

TABLE VII

DISTRIBUTION OF DIVIDENDS BY INCOME GROUPS*

Income group	Individuals	Dividends reported in million dollars	Individuals	Dividends reported in million dollars	Individuals	Dividends reported in million dollars
	1916		1917		1918	
Over $500,000	582	413.9	456	384.5	245	193.2
300–500,000	714	127.3	559	119.9	382	76.6
150–300,000	2,437	244.5	2,347	266.7	1,514	162.6
100–150,000	2,900	156.8	3,302	213.0	2,358	141.9
50–100,000	10,452	323.7	12,439	447.9	9,996	355.8
25– 50,000	23,734	326.6	30,391	473.4	28,542	428.6
10– 25,000	80,880	359.2	112,502	464.2	116,569	454.4
5– 10,000	180,553[1]	194.3[1]	270,666	279.9	319,356	326.2
3– 5,000	440,000[1]	163.0[1]	560,763	163.0[2]	932,336	202.0
Total over $3,000		2,309.3		2,812.5		2,341.3
under 3,000		165.0[1]		218.2[2]		233.6[2]
Total Dividends[10]		2,474.3		3,030.7		2,574.9
	1919		1920		1921	
Over $500,000	254	170.9	156	132.0	84	79.8
300–500,000	425	75.8	239	61.1	162	55.4
150–300,000	1,864	158.4	1,063	125.6	739	97.1
100–150,000	2,983	146.0	2,191	143.4	1,367	100.6
50–100,000	13,320	350.4	12,093	409.5	8,717	322.2
25– 50,000	37,477	426.7	38,548	525.5	28,946	425.7
10– 25,000	162,485	471.0	171,830	574.1	132,344	485.2
5– 10,000	438,851	322.0	455,442	386.0	353,247	349.2
3– 5,000	1,180,488	198.3	1,337,116	220.5	1,072,146	230.0
Total over $3,000		2,319.5		2,577.7		2,145.2
under 3,000		227.5[3]		251.8[3]		331.8
Total dividends		2,547.0		2,829.5		2,477.0
	1922		1923		1924	
Over $500,000	228	138.0	215	143.6	317	187.1
300–500,000	309	64.3	327	72.2	457	92.0
150–300,000	1,323	134.0	1,301	145.4	1,876	183.3
100–150,000	2,171	126.2	2,339	153.8	3,065	181.7
50–100,000	12,000	352.0	12,452	398.6	15,816	468.7
25– 50,000	35,478	437.9	39,832	520.9	47,061	555.1
10– 25,000	151,329	562.8	170,095	662.0	191,216	657.7
5– 10,000	391,373	356.3	387,842	346.1	437,330	292.1
3– 5,000	1,190,115	227.3	1,719,625	421.3	1,800,900	380.8
Total over $3,000		2,398.8		2,868.9		2,998.5
under 3,000		263.4		362.5[4]		352.1[5]
Total dividends		2,662.2		3,226.4		3,350.6

TABLE VII (*Continued*)

Income group	Individuals	Dividends reported in million dollars	Individuals	Dividends reported in million dollars	Individuals	Dividends reported in million dollars
	1925		1926		1927	
Over $500,000	686	264.0	699	361.6	847	406.7
300–500,000	892	119.1	892	151.0	1,141	171.7
150–300,000	3,223	252.3	3,267	301.5	3,873	331.1
100–150,000	4,759	225.9	4,724	271.1	5,261	276.5
50–100,000	20,958	512.5	20,520	578.8	22,573	623.8
25– 50,000	59,721	618.3	57,487	666.6	60,123	679.9
10– 25,000	236,779	731.9	246,730	815.4	252,079	834.5
5– 10,000	503,652	321.3	560,549	435.3	567,700	430.6
3– 5,000	1,880,000[9]	400.0[6]	1,880,000[9]	400.0[7]	2,120,000[9]	450.0[8]
Total over $3,000		3,445.3		3,981.3		4,204.8[10]
under 3,000		400.0[6]		400.0[7]		450.0[8]
Total dividends		3,845.3		4,381.3		4,654.8

[*] Figures for individuals by years taken from Statistics of Income, 1927, p. 25; figures for dividends by years derived from Statistics of Income for respective years.

[1] In 1916 the exemption limit was $3,000 for both married and unmarried, but it is apparent that the returns are not complete. Therefore $3,000–$5,000 dividends were assumed to be in the same ratio to $5,000 or over dividends as the average for 1917 and 1918 or 7.8 per cent. In the same way under $3,000 dividends were assumed to be in the same proportion to all dividends over $3,000 as the average of 1917 and 1918 or 7.3 per cent. The individuals in the $3,000–$5,000 group were assumed to be in the same proportion to individuals in the over $5,000 group as the average for 1917 and 1918 or 16.2 per cent. Also 50.0 million dollars and 30,000 individuals were arbitrarily added to the $5,000–$10,000 group to make up for possible failure to file returns.

[2] Dividends reported by the class $2,000–$5,000 in 1917 were prorated to the classes under $3,000 and $3,000–$5,000 in the same proportion as the average from 1918 to 1923 or 70 per cent to the $3,000–$5,000 group.

[3] For the group under $3,000 in 1917–1920 an adjustment was made by arbitrarily adding 100.0 million dollars to dividends to account for the dividends received by individuals not required to report.

[4] Of the total dividends reported as paid by corporations in 1923, all but $172.6 mil was reported as received by individuals or corporations. Of this remainder 100.0 million dollars was arbitrarily attributed to dividends received by the under $3,000 group. This seems reasonable, since the $1,000 to $3,000 group increased greatly over 1922 and the under $1,000 dropped over 75.0 million dollars.

[5] Of the total dividends reported as paid by corporations in 1924, all but $173.6 mil. was reported as received by individuals or corporations. Of this remainder 100.0 million dollars was arbitrarily attributed to dividends received by the under $3,000 group.

[6] Because of the raising of the exemption limits in 1925, $969.7 mil. of dividends reported as paid by corporations was not reported as received by corporations or by individuals with incomes over $5,000. Eight hundred million of this amount was attributed to the under $3,000 and the $3,000 to $5,000 groups in the same proportion as the average of the previous three years, or 50 per cent to each.

[7] In 1926, $857.9 million of dividends paid by corporations was not reported as received by corporations or by individuals with incomes over $5,000. $800 mil. of this amount was divided as in (6) between under $3,000 and $3,000 to $5,000.

[8] In 1927, $1,003.0 mil. of dividends was unaccounted for. $900 mil. of this was divided as in (6) and (7).

[9] Same ratio of individuals to dividends assumed as the average for the same income class from 1921–1924.

[10] The total dividends received by individuals as shown by this table differs very markedly in certain years from the estimate of dividends received by individuals recently published by the National Bureau of Economic Research in the National Income and its Purchasing Power (Willford I. King, New York, 1930).

The discrepancy seems to arise, in large part, from two facts. First, the figures that are given in the present table do not purport to be the same as those of Dr. King. The present figures do not include dividends paid by domestic corporations to foreigners; and, further, the assumption has been made that a considerable amount of failure to report dividends received occurred among all income classes in the earlier years, particularly in 1916. Therefore, the total dividends reported in income-tax returns, even if the lower brackets were included, would give a smaller figure than the total dividends paid to individuals by corporations.

A second cause of discrepancy between the two sets of estimates appears to be due to a weakness in the method employed by Dr. King in making certain of his estimates for the years before 1922.

For those years he assumed that the same rate of dividends was paid on all stocks as was paid on the outstanding stock of a sample group of corporations. Unfortunately, the stock of very large corporations composed the bulk of the sample. Since the large corporations are known to have been very much more prosperous than the average corporation, and presumably paid larger dividends, the sample is not representative. It would naturally lead to a very considerable overestimate in certain years. Dr. King has acknowledged the weakness in the method.

The difference between Dr. King's estimates and those used as the basis for the present discussion does not, therefore, impair the validity of the conclusions here drawn.

TABLE VIII

DIVIDENDS RECEIVED BY DIFFERENT INCOME GROUPS[1]

Income groups		1916[2]	1917	1918	1919	1920	1921	1922	1923	1924	1925	1926	1927
Total dividends in millions of dollars													
						(a)							
Largest	1,000	475	505	310	290	275	225	260	290	305	320	420	445
Largest	5,000	825	895	605	540	540	460	520	555	605	645	825	860
Largest	25,000	1,385	1,550	1,140	1,035	1,055	910	980	1,075	1,190	1,280	1,575	1,635
Largest	100,000	1,890	2,195	1,660	1,520	1,635	1,385	1,545	1,695	1,875	2,045	2,410	2,520
Largest	500,000	2,240	2,675	2,150	2,045	2,270	1,890	2,125	2,370	2,525	2,890	3,340	3,500
Largest	1,000,000	2,350	2,805	2,280	2,215	2,450	2,030	2,280	2,575	2,720	3,095	3,630	3,800
Total		2,424	3,031	2,575	2,547	2,829	2,477	2,662	3,226	3,351	3,845	4,381	4,655
						(b)							
Largest	1,000	475	505	310	290	275	225	260	290	305	320	420	445
Next	4,000	350	390	295	250	265	235	260	265	300	325	405	415
Next	20,000	560	655	535	495	515	450	460	520	585	635	750	775
Next	75,000	505	645	520	485	580	475	565	620	685	765	835	885
Next	400,000	350	480	490	525	635	505	580	675	650	845	930	980
Next	500,000	110	130	130	170	180	140	155	205	195	205	290	300
Other than	1,000,000	74	226	295	332	379	443	382	651	631	750	751	855
Percentage of total dividends received by individuals													
						(c)							
Largest	1,000	19.6	16.7	12.0	11.4	9.7	9.1	9.8	9.0	9.1	8.3	9.6	9.6
Next	4,000	14.5	12.9	11.5	9.8	9.4	9.5	9.8	8.2	9.0	8.5	9.2	8.9
Next	20,000	23.1	21.6	20.8	19.4	18.2	18.2	17.3	16.1	17.5	16.5	17.1	16.7
Next	75,000	20.8	21.3	20.2	19.0	20.5	19.2	21.1	19.3	20.4	19.9	19.1	19.0
Next	400,000	14.4	15.8	19.0	20.6	22.4	20.4	21.8	20.9	19.4	22.0	21.2	21.0
Next	500,000	4.5	4.3	5.0	6.7	6.4	5.7	5.8	6.4	5.8	5.3	6.6	6.4
Other than	1,000,000	3.1	7.4	11.5	13.1	13.4	17.9	14.4	20.1	18.8	19.5	17.2	18.4
		100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
						(d)							
Largest	25,000	57.2	51.2	44.3	40.6	37.3	36.8	36.9	33.3	35.6	33.3	35.9	35.2
Next	75,000	20.8	21.3	20.2	19.0	20.5	19.2	21.1	19.3	20.4	19.9	19.1	19.0
Other than	100,000	22.0	27.5	35.5	40.4	42.2	44.0	42.0	47.4	44.0	46.8	45.0	45.8

[1] Derived by interpolation from Table VII; for method see text.

[2] In 1916, the returns of husband and wife filing separately were combined by the Treasury Department and reported as one individual. An adjustment was made for this, based on the number of wives filing separate returns. The figures given here are comparable for those of subsequent years when husband and wife, reporting separately, were compiled as two individuals.

DEMAND CURVES IN THEORY AND IN PRACTICE[1]

SUMMARY

I. The use of theory in the interpretation of statistical demand curves, 601. — II. The equilibrium analysis; its assumptions; the difficulties of applying it to statistical curves, 603. — III. Dynamic curves interpreted with the time element left in, as (*a*) paths of equilibrium, on the basis of the equilibrium analysis; (*b*) moving schedules, with different assumptions. Marshall's view, 608. — IV. A possible application of the static analysis; all time elements eliminated from the statistical curves; Moore's curves and their meaning; dynamic curves not demand curves in the orthodox sense; the concept of elasticity not applicable to such curves; present statistical curves neither dynamic nor static; not to be interpreted in terms of orthodox theory, 614.

I

Over fifteen years ago the pioneer work on the derivation of demand curves from statistical data was begun by Professor H. L. Moore. Since then the subject has progressed considerably, but more from a technical than from a theoretical angle. Methods of deriving such curves have been refined. We have now the multiple correlation analysis, various more certain methods of eliminating time, Schultz' method of "normals," Leontief's method of coördinated lines, and the like. I shall not go into the technical methods in any detail. Those whose interest is principally in the questions of

1. Some of the suggestions made in this paper were included in a short discussion of price-forecasting methods, made by the author at the joint meeting of the American Statistical, Economic and Farm Economic Associations at Washington, December, 1929. This discussion has been printed in the minutes of the Journal of Farm Economics. I have also to acknowledge the helpful criticisms of Professors W. L. Crum, and J. H. Williams, Mr. A. G. Silverman, and Dr. Holbrook Working.

method will easily find all needed explanation in the designated article.[2] The present paper is concerned chiefly with the interpretation of the demand curves arrived at by the various methods. Mr. Philip Wright wrote in 1915 that "Professor Moore's studies in demand curves illustrate the principle that the need of checking statistical inductions by abstract reasoning is quite as great as that of verifying abstract reasoning by statistics."[3] That same principle has been amply illustrated ever since by the studies not only of Professor Moore but of his many successors.

The use of theoretical terms in connection with the interpretation of statistical investigations has recently become common both with theorists and business men. Unfortunately the assumptions upon which the theory itself has been built up are usually disregarded. Many have failed to recognize that the hypotheses may be so out of line with the assumptions necessary for the use of actual economic data, that they cannot be used. Confusion has, therefore, resulted from the indiscriminate application of the conclusions of economic theory to the results of the analysis of economic problems.

Such confusion can be avoided, I think, and the work of theorists and practical statisticians alike aided by the examination of the fundamental assumptions of "orthodox" economic theory, and the differences in the assumptions necessary in the use of actual economic data.

2. A good bibliography of the earlier work is given by Holbrook Working in the Quarterly Journal of Economics for August, 1925, and also by Warren and Pearson, "Interrelationships of Supply and Price," 1927. To these may be added Dr. Leontief's article in the July, 1929, number of the Weltwirtschaftliches Archiv; "Die Analyse von Nachfragekurven" by Dr. Staehle, published by the Frankfurter Gesellschaft für Konjunkturforschung, 1929; and "Sopra Alcuni Problemi di Dinamica Economica" by Vincenzo Moretti, in the Giornale degli Economisti for July, 1929.

3. Review of Moore's Economic Cycles by P. G. Wright, Quarterly Journal of Economics, May, 1915.

The need for this sort of analysis has been previously recognized and considerably forwarded by individuals such as Dr. Holbrook Working,[4] Mr. E. J. Working,[5] Dr. Ezekiel,[6] Professor Schultz,[7] Mr. P. G. Wright,[8] Dr. Leontief,[9] and V. Moretti.[1] It has not always been carried to its logical conclusion, however, as I shall hope to show. Certainly the last word has not yet been said upon the relation between theoretical economics and the interpretation of statistical analysis.

II

Since the time of Cournot and especially of Marshall, both demand and supply have been thought of as schedules; the one (demand) expressing the amount of a single commodity which would be bought by consumers at various prices; the other (supply) expressing the amounts which would be sold by producers at various prices. These schedules apply, in theory, to an instant of time, and to a given market. At a different instant of time, or in another market, they would move either to the right or left

4. "The Statistical Determination of Demand Curves," Quarterly Journal of Economics, August, 1925.

5. "What Do Statistical 'Demand Curves' Show?" Quarterly Journal of Economics, February, 1927.

6. Quarterly Journal of Economics, May, 1927; also "Factors Related to Lamb Prices," Journal of Political Economy, 1927.

7. Statistical Laws of Demand and Supply, University of Chicago Press, 1928.

8. The Tariff on Animal and Vegetable Oils, 1928, Appendix B; also review of Schultz' Statistical Laws of Demand and Supply, Journal of the American Statistical Association, June, 1929.

9. Op. cit., p. 1.

1. Giornale degli Economisti, op. cit., July, 1929. Moretti summarizes in this article the work of American economists on demand curves, from Moore to E. J. Working and Ezekiel, including also the contributions of mathematicians such as Ross, Evans, and his countryman Amoroso. In Moretti's criticisms and in the course of his own analysis, he has suggested several of the points which occurred to me quite independently. Not until the first draft of this article had been written was Moretti's work brought to my attention. The points of agreement and disagreement between us will be indicated throughout this article.

with changing inclinations.[2] The quantity to be bought and sold under these assumptions is therefore an amount already produced or a stock in hand at that instant, and the demand and supply schedules express the proportion of this stock which would be bought and sold at various prices. The point at which the demand and supply schedules intersect is the price at which the entire stock can be sold at that time and in that market.

The Marshallian version of the equilibrium analysis makes another assumption as well. The prices and quantities represented by the supply and demand schedules apply only to *one* commodity, and the prices of all other commodities are assumed constant. It is the well-known assumption of *ceteris paribus*. The mathematical economists—Walras, Pareto, and the like—do not make this assumption, and allow for the changes in the prices of other goods through a series of simultaneous equations. Their analysis still applies to a moment of time, however, and to a perfect market.[3]

The precise definition of elasticity really dates back to Cournot, as Schultz has pointed out,[4] altho the more

2. It is important to note that the slope of the schedules, consequently their elasticities, may change in any way over time. The diagrams of most theorists, in that they picture either rectangular hyperbolas or straight lines, and usually assume parallel shifting, are apt to make one forget that the assumption of constant elasticity is not necessarily in accord with actuality.

3. Walras starts from two equations expressing the exchange relationship between two commodities which he calls "l'équation de demande partielle" and proceeds by logical steps to the setting up of $(m - 1)$ simultaneous equations expressing the relationship between the quantities and prices of all commodities. He assumes that the demand for one commodity is the supply of another, and the two are equal at the equilibrium price. The mutual relationship between the demand and supply and price of all commodities is, therefore, expressed by the Walrasian equations. Eléments d'Economie Politique Pure, 1889 (2nd ed.), Lessons 9 and 11.

4. Statistical Laws of Demand and Supply, 1928, pp. 6–7. See Cournot, Mathematical Principles of Theory of Wealth (W. J. Ashley, ed.), pp. 53–54.

familiar statement comes from Marshall: "The *elasticity* (or *responsiveness*) of *demand* in a market is great or small according as the amount demanded increases much or little for a given fall in price, and diminishes much or little for a given rise in price." [5] Elasticity, is, then, the rate at which the amount of a commodity which *would* be bought or sold changes with given changes in price, when these changes are very small. Implicit in this idea of elasticity, it seems to me, is the fact that it applies to a given instant of time and to a given market. It does not refer ordinarily [6] to the movement of quantity and price over time. It is rather a measure of the changing slope of the instantaneous demand schedule — a hypothetical curve to which, by the very terms of the definition, statistical data cannot correspond. That this interpretation is not unfounded is shown by the use of the word *would* in the analysis of elasticity changes, both by Marshall and others. For instance, "If a fall in price from say 16*d.* to 15*d.* per pound of tea *would* much increase his purchases, then a rise in price from 15*d.* to 16*d.* *would* much diminish them. That is, when the demand is elastic for a fall in price, it is elastic also for a rise." [7] In other words, this is what *would* happen at a moment of time, altho it cannot be empirically ascertained because of our inability to set forth the instantaneous demand schedule.[8] Using

5. Principles (8th ed.), p. 102.

6. Marshall is somewhat confusing here, as he sometimes appears to be thinking of elasticity *over time* — a very different thing. See bk. III, chap. 4. See also on this point, P. G. Wright, The Tariff on Animal and Vegetable Oils, Appendix B, p. 293.

7. Marshall, p. 102. Italics are mine.

8. The mathematical economists have brought out most clearly the psychological nature of the instantaneous schedule. It is, in fact, a utility curve usually expressed in terms of price for the purposes of the market analysis. For example, Pantaleoni remarks: "I use the term *demand* advisedly, in order that the reader may have the opportunity of making himself acquainted with the precise import of this word in eco-

"static" to describe the cross-section, instantaneous conditions, elasticity is, therefore, tied to the assumptions made in the static analysis.

Both economists and statisticians have wrestled with the application of the equilibrium analysis (as stated in its most rigid terms above) to curves statistically derived. The chief difficulty has been emphasized again and again — that of time. In its cross-section sense, the static analysis denies the entrance of time variation. There is, however, a sense in which static is sometimes used in which the element of time does enter; that is, time variation is allowed to take place, but only in a proportional and predictable way. The original relationship between the variables remains the same, altho the position of the whole may change.[9] This change is comparable to a shift in the demand schedule, for example, while the elasticity is assumed to remain the same. The schedule, as a cross section of economic relationships, assumes different positions in the eco-

nomics. Here demand is to be understood in the sense of *scale of the degrees of utility* of successive increments of a commodity, and a *variation in the demand* consists of a *variation in this scale*, consequent on a *variation in the wants and tastes of consumers.*" (Pure Economics, 1898, p. 148.) Both Moretti (op. cit.) and L. Amoroso bring this out. Cf. Amoroso ("Le equazioni differenziali della dinamica economica," Giornale degli Economisti, February, 1929): "The law of demand is therefore not an empirical law. *It is a mental category* which has its origin in the intuition of the psychological foundations of our economic conduct." Having concluded that "there does not therefore exist a problem of the *experimental verification of the law of demand*, or of the *empirical search for the law of demand*," he somewhat inconsistently spends the rest of his paper devising dynamic laws of demand, without making it quite clear that they are distinct from the usual orthodox laws of demand. (The italics are in the original.) Cf. P. G. Wright in his review of Schultz' book (Journal of the American Statistical Association, June, 1929) p. 210.

9. See Marshall's discussion of the stationary state, bk. V, chap. 5. His description of the representative firm is a case in point. Moretti (op. cit.) thinks this moving static situation more typical and quotes the example of Clark's "tranquil body of water," in which there is "perfect fluidity but no flow." I do not think, however, that the rigidly logical analysis of demand does take time into account even in this way.

nomic system over time, but it does not change within itself. The usual definition of static, however, may be said to be that of the cross section, in the ordinary use of the equilibrium analysis.

Time variation, as it affects economic phenomena in actuality is not regular, or as yet predictable in its effects. Almost invariably it upsets the relationship existing at any one moment between the variables. It brings into action all sorts of new and unaccountable elements, and affects the relative rates of changes of the variables in diverse ways. Whenever statistical data are used, the question of time must always be considered, for they necessarily refer to the changes in variables at successive instants of time. They may also refer to variation in successive places at the same instant of time. This may be disregarded for the present discussion, however, as price-quantity data used for demand curves takes prices and quantities of a commodity over time, rather than over regions.

There is another difficulty besides that of time, in applying the equilibrium analysis to statistically derived curves. In the static equilibrium the quantity under consideration is a supply already on the market, or at least available for the market; a stock already produced. The idea of cost of production, in the sense of the physical cost of producing changing amounts of a commodity, under different productive conditions, does not enter. The quantities which a statistician must use however, are quantities produced under varying conditions of production, at changing costs, at different instants of time. Statistically speaking, quantity varies as well as price, over time, whereas in the static analysis the quantity is fixed.[1]

1. Cf. P. G. Wright, who assumes that the quantity is "indefinite," op. cit., p. 290.

Most of the attempts to reconcile orthodox theory with statistically derived curves have been concerned with the modification of static theory so as to fit the actual data, or with an attempt to modify the data, primarily through the elimination of time so as to fit the theory. There has been, for the most part, no question that orthodox theory could be applied successfully to the interpretation of statistical curves. And yet it has been repeatedly remarked that the theoretical analysis does not include time and the statistical data do. What is more, it is admittedly impossible to eliminate time completely from statistical series. It seems strange that so few attempts have been made to attack the problem theoretically with the element of time left in.[2]

III

If the element of time is included, it seems to me that the statistical curves can be interpreted in two different ways: one quite consistent with the static equilibrium analysis, the other from an entirely different approach.

1. In a series of prices and quantities applying to successive instants of time, we have only one price-quantity point for each instant. This point may be assumed to be the point of intersection of the supply and demand schedules for that instant of time. We are then dealing with points of successive static equilibria, in the price-quantity distribution over time. In Figure 1, a, b, c, and d are the points of equilibria of the successive instantaneous schedules; 1, 2, 3 and 4 are also equilibrium

2. Professor Moore has tried to do this in his articles on dynamic demand curves and a moving economic equilibrium. I shall deal with these specifically later on. Holbrook Working and M. Ezekiel, in the articles previously mentioned, have also given attention to the theoretical aspects of this problem. There is a growing attention to the problems of dynamic economics by the younger followers of the mathematical school, among whom may be mentioned Evans, Roos, Amoroso, and Moretti.

points. Which of the two curves is to be used depends on the direction of the shifting of the schedules. This assumption has been made, quite logically I think, both by Mr. E. J. Working and by Dr. Leontief.[3] The curve fitted to these equilibrium points is neither a demand nor a supply curve, but a path of equilibrium. Neither Mr.

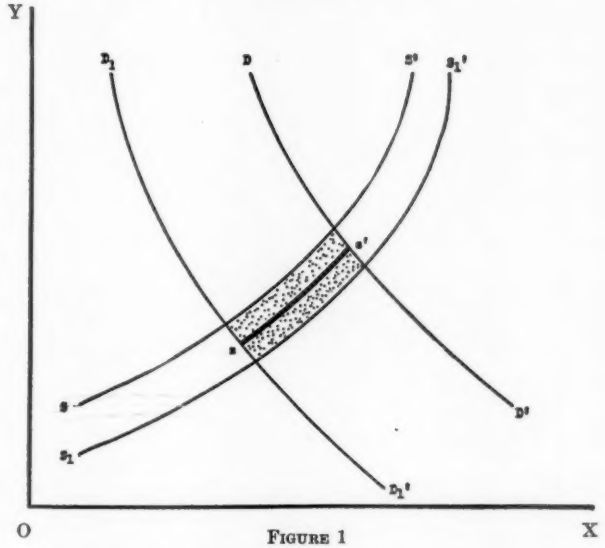


FIGURE 1

Working, nor Dr. Leontief take this final, and it seems to me, inevitable step.[4] Mr. Working comments on the

3. Quarterly Journal of Economics and Weltwirtschaftliches Archiv, articles previously cited.

4. Dr. Leontief, in fact, develops quite a different angle of approach which will be considered in a later article on the method he sets forth. Moretti (op. cit.), in analyzing Working's article, accepts his conclusion that the final curve is some sort of demand curve. Amoroso, however (op. cit.), seems to have an idea similar to mine, in mind. He calls the fitted curve a "historical trajectory," however, instead of a path of equilibrium.

difficulty of deciding just what the final curve is. He shows that if the changes over time are largely on the supply side, the curve is negatively inclined, and vice versa. (Cf. Figs. 2 and 3.) It has always seemed illogical to me that such a curve should be called a demand curve when its slope is determined by changes in supply. If

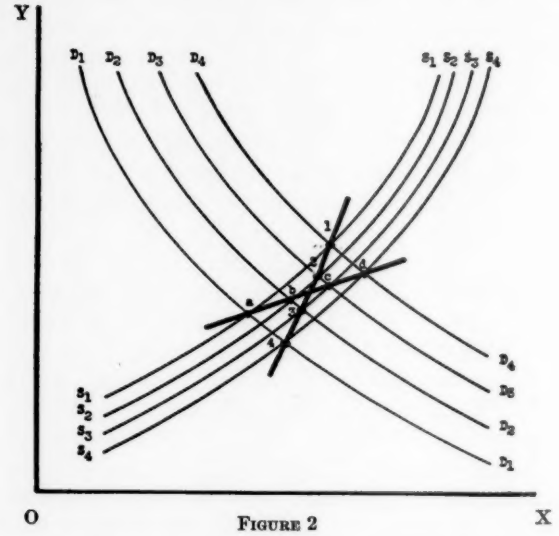


FIGURE 2

one accepts the idea that the curve is a path of equilibrium, this logical difficulty disappears. In Figures 2 and 3, *dd* and *ss* are not demand and supply curves at all, as both Working and Moretti state, but paths of equilibrium.[5] The curve may be inclined in any way,

5 On this point, see Wright, op. cit., p. 296. Only when either the demand curve or the supply curve does not shift nor change its elasticity over time, is the resulting curve a demand or a supply curve. (Cf. the diagrams in Leontief's article). In that case, the demand curve or the supply curve is the path of shifting of supply or demand respectively.

depending on the balance of forces making for changes in equilibrium.

This analysis is perfectly consistent with the static theory of equilibria, yet it allows for the entrance of the time factors. It puts the emphasis on the changes in the equilibrium points, however, and not on the changes in

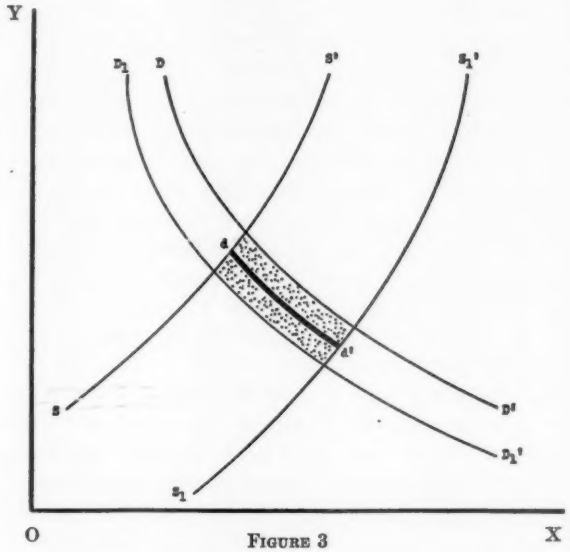


FIGURE 3

the demand and supply schedules themselves. Ultimately, of course, the changes in equilibria are related to changes in the demand and supply schedules. If the equilibrium point has moved, obviously the demand or the supply schedule or both of them have moved as well. But how can these successive instantaneous schedules be determined, or, in other words, how can the continual changes in elasticity and the shifting of the demand and

supply curves be measured over time? Before the elasticity or shifting of a curve can be measured, the curve must be known; but, on the above assumptions we know only the point of intersection of the two schedules. We cannot, therefore, determine the elasticity or shifting.[6] From a statistical point of view, however, there is this to be said. All the points on the scatter diagram do not fall upon the fitted curve which is the path of equilibrium. The scatter of points about the fitted curve is to some extent, therefore, a measure of the elasticity and shifting changes of the instantaneous schedules. For, if the elasticity of both demand and supply were constant over time, if their shiftings were parallel and all in one direction and at constant rates, the equilibrium points would lie on a straight line. Their deviations from that straight line, therefore, indicate the occurrence of elasticity and shifting changes.[7] The difficulty lies, of course, in separating elasticity changes from those of shifting, and in differentiating changes on the demand from changes on the supply side.

2. It is also possible to approach demand and supply over time as schedules made up of a series of moving points. In that case, each price-quantity point is a point referring to a single instant of time, and represents the successive prices at which constantly varying amounts of a commodity, produced under changing conditions of production, have actually been bought or sold. The supply curve, under these assumptions, indicates the different quantities of a commodity produced

6. Unless the nature of the elasticity and shifting are assumed. Cf. Leontief, who assumes that elasticity is constant and that shifting is parallel on a logarithmic scale (op. cit.). Cf. also Ezekiel's discussion of this point (op. cit. pp. 207–212). He, too, assumes constant elasticity, and remarks that unless this assumption is made, the underlying demand curve cannot be derived from data extending over time.

7. Cf. Staehle's idea that the scatter about the fitted line marks the path of shifting of demand, op. cit., p. 37.

under varying conditions at successive points in time and sold at varying prices, and not, as in the static anal-analysis, the psychological cost of relinquishing changing amounts of a stock already in existence at a single instant, as the result of a hypothetical change in price. In the same way, a demand curve of this nature represents the amounts of this variable quantity sold to consumers at successive instants of time. The consideration of demand and supply schedules in this way, as curves built up thru time, necessitates that (1) both price and quantity be considered as variable; (2) time be left in; (3) the static equilibrium approach be discarded. From this point of view, the elimination of time elements from the statistical data in deriving demand curves is questionable, as is, as well, the application of many of the concepts of orthodox theory.

I do not wish to indicate that these two approaches have not been thought of previously. Marshall quite evidently had both of them in mind. It is a little difficult, however, to be sure when he is thinking in terms of moving equilibria, and when in terms of the dynamic schedules described above. In his discussion of the effects of time on *both* demand and supply,[8] he seems to be considering the change in the equilibria over time. But in a later analysis of the effect of time on supply, and of the various meanings of "normal" as applied to the supply schedule,[9] his analysis is clearly in terms of a schedule such as I have described above. Marshall does not explicitly differentiate between these approaches. Indeed, altho he recognizes the difficulty of applying static hypotheses to a situation in which time is the prime factor, he never questions that it can be done, by some process of gradually modifying the original assumpsumptions. He does not appear to see that frequently

8. Principles, bk. V, chap. 1. 9. Principles, bk. V, chap. 5.

the situation shown by statistical data is such that entirely different assumptions must be made.[1]

IV

The last few paragraphs have been concerned with the attempt to develop possible theoretical approaches to the study of demand curves, with the element of time included. Such analyses can be applied to statistical price-quantity data and can be used to interpret statistically derived curves. But for this purpose *time must be included and not eliminated* as a factor in the equations for the statistically derived curves.

There is a way, however, in which the orthodox static analysis has been applied to statistical data, at least approximately, and with very little modification. If the instant of time is extended to a period of time, and during that period all time variation, in so far as it is technically possible, is removed, the curve fitted to the corrected data is considered a sort of average supply or demand schedule, in the orthodox sense, for that extended instant of time. Probably Cournot had this sort of thing in mind when he recommended the use of the annual average price in the expression $F(p)$.[2] It is evidently what Marshall meant when he said: "We desire to obtain, if possible, a series of prices at which different amounts of a commodity can find purchasers during a given time in a market." [3] From this point of view it is

1. This confusion is not confined to Marshall, but exists in the work of most writers on the subject, who are not in the habit of stating their assumptions. The mathematical economists do that, at least, whatever else their shortcomings.

See also the discussion of supply curves by Wright, op. cit., p. 292.

2. Op. cit., p. 52. See also Schultz, op. cit., who attempts to set up a theory by which the static equilibrium may be approached. The logic of this analysis seems doubtful and will be given more critical attention in an article under preparation.

3. Principles, p. 112.

considered possible to approximate an average static situation within a given interval of time. Dr. Ezekiel was apparently thinking in the same terms, when he indicated that the static curve of theory could be approached by the use of an average curve.[4] The elimination of time means that the result represents the average conditions within the interval selected, and not that of any other interval — one year, or month. Such a demand schedule would indicate that during that period of time certain amounts of a commodity could be sold at certain prices, and the average elasticity of demand for that period could be determined. It would not indicate that at a specific year within the period, certain amounts were sold at certain prices, or that a specific elasticity of demand applied to a given year. In other words, in this type of analysis the length of the period itself is taken as the point in time, the demand and supply schedules are approximately instantaneous, and any units of time within the period must be disregarded.

The elimination of time is, of course, the crux of the problem. If one eliminates time, what sort of results are obtained and how may they be interpreted? Most statisticians remove some varities of time variation, such as seasonal or secular, but leave cyclical and random variations untouched. The resulting curves have been called both static and dynamic. What is more, the orthodox concept of elasticity has been applied to them without question. It is important that a simple and elementary statement of the factors involved be made. If time is not entirely eliminated, the resulting curve cannot be called a demand curve.[5] It is either a path of equili-

4. See his article in Quarterly Journal of Economics, February, 1928.

5. Moretti and Amoroso have both come to this conclusion: that statistical and theoretical "demand" curves have nothing in common; and that if one is going to use the terms of economic theory, the statistical curves cannot be called demand curves.

brium or a moving curve based on assumptions differing markedly from those of the orthodox analysis. If time *is* eliminated, as completely as statistical methods allow, the curve approaches the static concept according to the above analysis. Most of the actual curves which have been derived are hybrids, and it is difficult to know what they mean and how they are related to orthodox theory. Take, for example, the demand curves derived by Professor H. L. Moore, which he calls dynamic.[6] These are derived from series in which the ratio of the actual items to the ordinates of trend are substituted for the uncorrected data. I agree with Mr. E. J. Working that these so-called dynamic curves are rather static curves, in so far as secular influences are removed.[7] The same argument applies to Professor Moore's moving equilibrium, in which he substitutes trend ratios for the uncorrected data in the Walrasian equations.[8] Professor Moore contends that by so doing he has moved one step away from the static state assumed by Walras, and has taken secular influences into account. He calls the equations developed in this way expressive of a long-period moving equilibrium. I should say again that he has merely approximated the static situation implicit in Walras by eliminating one element of time variation, namely, secular trend.[9]

It is important for the statistician to decide whether

6. See Economic Cycles (New York 1914), chap. 4; "The Elasticity of Demand and Flexibility of Prices," Journal American Statistical Association, 1922; "A Moving Equilibrium of Demand and Supply," Quarterly Journal of Economics, May, 1925; "Partial Elasticity of Demand," Quarterly Journal of Economics, May, 1926.

7. See Working, op. cit., Quarterly Journal of Economics, February, 1927.

8. "A Theory of Economic Oscillations," Quarterly Journal of Economics, November, 1926.

9. All the comments on Moore's work that I have seen (cf. Amoroso and Moretti, op. cit.) remark that he has taken time into account implicitly. It seems to me that he has *eliminated* time, in so far as he could.

he wants a dynamic [1] or a static curve. His answer will determine whether or not time variation is to be eliminated. If he decides that a dynamic curve is most useful for his purposes, he has to realize that the curve resulting from his statistical procedure is not a demand curve in the orthodox sense. If he desires to use the logic of orthodox theory, it is a path of equilibrium; if he prefers to throw over this theoretical structure and build up a new set of theory from empirical observation, it is a dynamic schedule built up in a very different way from the schedule of the static equilibrium.

Does it matter what he calls the curve as long as it is useful? It does matter, in so far as he applies terms of theory which cannot be used, and confuses both himself and the theorist. Let me illustrate. For the purposes of price-forecasting the producer is interested in the relation between quantity and price over time. It is fairly obvious that he is interested in a dynamic situation and the changes brought by time. He wants an equation expressing this relation between variable prices and quantities, from which price can be estimated. He does not care whether the curve is called a path of equilibrium or by any other academic term. He wants a price forecast which is sufficiently accurate to enable him to base his business policy upon it.

Usually, however, the person who is forecasting for the producer does not stop there. Having derived his equation, expressing the price-quantity relationship over time, he then proceeds to talk about the elasticity of demand — to be procured from the same equation. I believe that here he falls into error. Elasticity, as the economist means the terms, is inevitably tied to the

1. Dynamic is used here in the sense of any change over time. It may be a regular and predictable variation, or it may be an irregular change, which completely rearranges the original set-up of the variables.

assumptions of the static state. The concept obviously cannot be applied to the dynamic curve considered as a path of equilibrium. Neither can it be applied to the moving curve. Mathematically, a coefficient of elasticity can be derived for such curves, but the result is *not* a coefficient of elasticity, as Cournot or Marshall or any orthodox economist of today uses the term. It may be something even more useful or important — but it ought not to be called elasticity.[2]

An average coefficient of elasticity can be found, however, in connection with a curve from which all elements of time variation are removed. This coefficient applies to the average relationship between quantity and price for the whole period under consideration, and not to any point of time within the period. Such a curve and such a coefficient describe a past situation, and can be used only with great caution, if at all, to forecast what will happen. They are related to an interval of time, which has been made to approximate, in so far as is statistically possible, the instant of time of the theorist. The results obtained, therefore, cannot be applied to any other such "instant" of time.

In conclusion, the analysis of statistical curves in relation to economic theory is necessary and desirable. It is not enough to derive a series of curves without knowing what they mean. Statistical method must be related to intelligent hypotheses if it is to be more than mechanical manipulation. From the preceding discussion, it is clear that both dynamic and approximately static

2. I should, therefore, say that the so-called coefficients of elasticity derived for various commodities by many economists are not coefficients of elasticity at all and cannot be used to apply the logic of orthodox theory to these commodities. Cf. P. G. Wright, op. cit., p. 293.

curves may be derived from statistical data.[3] The dynamic curves, no matter whether orthodox theory or any other theory is used for their interpretation, are not demand curves, and the concept of elasticity cannot be used in connection with them. This is of some importance, for it is the dynamic curves which are used for price-forecasting and from which elasticity has been derived. It is obvious, I think, that the same curve cannot be used for both purposes. It need not matter to the producer that the fitted curve, which he has called a demand curve, is not, theoretically, a demand cucve at all, as far as his forecasting price is concerned. But it does matter in so far as he desires to find the elasticity of demand for that commodity. He must derive another curve and by a different method.

The producer's interest in elasticity is secondary, but it is real. Having a price forecast he is anxious to know

3. Static curves can be approximated by making certain assumptions as to the movement of supply and demand over time. Dynamic curves can be derived by leaving all time elements in, or by some mathematical method of taking account of time. Mathematical economists are dealing with this problem, and both Roos and Amoroso have formulated differential equations of demand and supply in which time is explicitly taken into account. Roos ("A Dynamical Theory of Economics," Journal of Political Economy, October, 1927) develops the following equation of demand, in which demand is considered as a function not only of its price and the prices of all other commodities, but also as a function of the rate of changes of price over time, the accumulation of these effects, the rates of production, their acceleration, and the cumulation of these effects: —

$$G\ (u_1,\ u_1',\ -\ u_n,\ u_n',\ p,\ p',\ t) = \int_{to}^{t} P\ (u_1,\ u_1',\ -\ u_n,\ u_n',\ p,\ p',\ t,\ \tau)$$

It will be noticed that time (t) is introduced directly into the equation.

Amoroso (op. cit.) comes out with the following differential equations of demand and supply:

$$\frac{p'}{p} + a\,\frac{x'}{x} = \gamma$$

$$p' + hp = a + bx + cx'$$

in which p' and x' are the differentials of price and quantity with respect to time.

Theoretically these mathematical attempts to solve the problem of dynamic curves are interesting, but the mathematics is too involved to make such equations of any actual use, as the originators admit.

how much more or less can be sold at that price. If the price is lower than the preceding year, whether or not he can sell a greater quantity, without driving the price still lower, will depend on the elasticity of demand. Not only does elasticity interest the producer, but it is of greatest moment to theorists. The idea of elasticity permeates practically the entire field of economic theory. The attempt to test this theory is considerably affected by the idea that elasticity cannot be derived from the usual statistical curves.

The present curves so readily produced by statisticians and business men are neither static nor dynamic for the most part. They are a queer mixture of the two. They may be practically useful, but they certainly cannot be interpreted in terms of orthodox theory.[4] The ordinary terms of the economist are misapplied, when used in connection with them. It is my contention that they would be distinctly more useful, to say nothing of being more intelligible, if their assumptions were clearly and simply defined, in order that they might be interpreted by orthodox theory, or be based on an empirical theory of their own.

4. It is of some interest that Moretti, who has come to the same conclusion — that statistical data cannot verify the theoretical analysis of orthodox economics — implies that statistical method is, therefore, of little use. With that I cannot agree. One might as well conclude that theory is of no use, as many "practical" economists have. It is rather a case of developing theoretical reasoning to fit the empirical facts; of modifying, and, if necessary, discarding a *particular* body of theory, but certainly not deduction in general.

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FACTORS IN INDUSTRIAL INTEGRATION

SUMMARY

The apparent conflict of opinion among present writers. — I. The difficulties in the statistical measurement of integration, 622. — Recent American experience, 623. — II. The economies of integration under stable and competitive conditions, 624. — The incompatibility of technical balance and commercial self-sufficiency, 626. — III. Integration in relation to monopoly and dynamic conditions, 629. — The offensive and defensive aspects of integration, 632. — The vertical concern in periods of depression and periods of prosperity, 633. — The vertical firm and technical changes, 635. — IV. Conclusion: Conflicting views more apparent than real, 637.

THERE appears to be very much difference between authorities concerning the actual extent and the theoretical advantages of the grouping of successive industrial operations under one control. Whilst some believe that integration is becoming more common,[1] others consider that the movement is on the wane.[2] And, altho some argue that a business structure of this form brings only very limited economies which are offset by complexities in management,[3] the opposite view is held in recent American writings.[4] This divergence upon fact and theory is a reflection of the two fundamental difficulties in arriving at conclusions upon this question.

1. See Federal Trade Commission, Annual Report, 1929, p. 60.

2. Final Report of the Balfour Committee on Industry and Trade, p. 177. This Committee had, however, expressed quite the opposite view in an earlier report (Factors in Industrial and Commercial Efficiency, p. 13).

3. See Macgregor, Industrial Combination, p. 98; Lavington, "Technical Influences on Vertical Integration," Economica, March, 1927.

4. See Dewing, Financial Policy of Corporations, iv, 55; Frank, "Significance of Industrial Integration," Journal of Political Economy, April, 1925.

The first is that of making some statistical assessment of changes in the prevalence of integration. The second that of distinguishing between its advantages, on the one side, during periods of relative stability and free competition and, on the other, when monopoly influences are at work and important changes are taking place in the technique of supply or in the conditions of demand. To deal with these in turn:

I

The present division of the whole of productive activity into distinct industries is largely arbitrary, and often a matter of convenience in classification. Increasing integration between businesses in two industries may often measure disintegration which is going on elsewhere. If, for example, it becomes the practice to operate coke ovens in connection with blast furnaces, instead of in conjunction with coal mines, amalgamation on the one side is offset by disintegration on the other and the net effect cannot be crystallised into a generalisation concerning trends in industry as a whole. Technical changes may entirely eliminate a whole process in the chain of production and make a comparison between different periods impossible. In many industries the difference between the integrated and the horizontal firm is one not merely of size or structure but of class of product, and changes in demand may eliminate the one type or the other. In the iron industry "basic" iron is largely made by the firm which is integrated with a steel works whilst foundry iron is produced by independent blast furnaces.[5] A reduction in the demand for

5. Committee on Industry and Trade, Further Factors in Industrial Efficiency, p. 164, Productivity in Merchant Blast Furnaces, United States Bureau of Labor Report, No. 474.

foundry iron relative to that for basic would appear to constitute a growth of integration, tho this is obviously quite a different movement from that by which the integrated might succeed the present organisation in the production of foundry iron.

Whatever the difficulties of measurement, there can hardly be any doubt that, in the past twenty years in the United States at least, there has been a rapid growth of integration. That movement has perhaps been most marked in iron and steel where the number of blast furnaces independently operated fell from 164 to 84 between 1912 and 1926 whilst "steel works" blast furnaces rose from 104 to 226.[6] In the oil industry the units which were created by the 1911 dissolution of the Standard Oil Company are increasing their hold over the supplies of crude oil and the marketing facilities for refined products.[7] In the automobile industry the Ford Company and the General Motors Corporation are highly integrated and tend to become more so,[8] the latter having spread its control from motor-car engines, bodies, and accessories to electric power and light plants, refrigerators, aircraft, radio and patent motor fuels. In the motion-picture industry a few large firms now control production from the making to the exhibiting of the films. The copper industry is controlled, from mining to the manufacture of brass goods, by great integrated concerns which are also strongly interested in the power industry. Before its dissolution in 1923 the Packers Trust had, by a policy of conglomeration, moved out from meat buying and packing to the acquiring of public stockyards, retail meat stores, and production on a large

6. Bureau of Labor Report, No. 474, p. 138.

7. Federal Trade Commission, Petroleum Prices, Profits and Competition, 1928, chap. 3.

8. Seltzer, Financial History of the Automobile Industry, p. 58 *et passim*.

scale of grocery products of all kinds.[9] But even during this most rapid growth in industry at large, certain industries remained almost untouched by the movement. This is most clearly brought out by a comparison of some of the different sections of the iron and steel industry. Such scanty evidence as this cannot, of course,

INTEGRATION IN THE IRON AND STEEL INDUSTRY OF THE UNITED STATES

(Derived from material presented by the Bureau of the Census)

	Blast furnaces	Steel works and rolling mills			Wire industry
	Pig iron produced for sale[1] (percentage of total production)[2]	Pig iron purchased by steel works (percentage of total consumption)[2]	Steel ingots produced for sale (percentage of total produced)[2]	Finished rolled products and forgings for sale (percentage of total produced)[2]	Production by wire firms which purchased rods (percentage of total production)[2]
1904	40	18	..	..	..
1909	38	19	..	73	46
1914	33	12	3	68	53
1919	29	11	3	68	41
1921	23	..	2	63	39
1923	26	..	3	67	40
1925	24	..	2	68	40

[1] I. e. not used in the same or some other plant of the same company.
[2] Based on weight.
[3] Based on value.

lead to conclusive results but it is significant that, during a period when circumstances generally were fostering integration and in an industry which notoriously has favoured this structure, certain rigid limits to the movement appear to exist.

II

It is impossible to explain a movement towards integration such as that which is taking place at present in the United States without distinguishing between two sets of circumstances which appear to react quite differently upon the structure of industry. Integration under

9. Federal Trade Commission, Report on the Meat Packing Industry, 1919, Part IV, p. 24.

a condition of competition and generally stable conditions is one thing. Integration where monopoly exists and changes in demand or methods of production occur frequently is quite another.

Where stability and competition exist, the advantages of integration would appear to be limited. The vertical business creates increasingly serious complexities in management which weaken its efficiency.[1] And there are further definite limitations to the alleged gains of this form of organisation. The economies of integration are usually grouped into three: the seizing of intermediate profits; the improvement of technical processes by the introduction of continuous operation;[2] and the guarantee which is afforded of supplies of suitable raw materials[3] and markets.[4] Where competition exists, however, the dangers of the shortage of raw materials and markets, theoretically, do not exist; and the intermediate profits to be gained are no larger than the company might obtain in other directions as, for example,

1. Marshall, Industry and Trade, p. 216; Lavington, "Technical Influences in Integration," Economica, March, 1927.

2. As, for example, in bringing together of processes in the manufacture of iron and steel whereby heat is saved, or spinning and weaving whereby the right sort of yarn in suitable condition can be brought to the looms.

3. It is for this reason that the vertical firm is found in the woollen section of the Yorkshire wool textile industry (Clapham, Woollen and Worsted Industries, p. 148), in certain parts of the Lancashire cotton industry; to an increasing extent in the English automobile industry (G. C. Allen, London and Cambridge Economic Service, Special Mem., No. 18.); and the English iron and steel industry (Committee on Industry and Trade, Further Factors in Industrial and Commerical Efficiency, p. 88).

4. Thus the International Paper and Chemical Co. entered the publishing business to find a guaranteed outlet for its paper pulp; the association of iron and steel works enables the steel works, in slack times, to engage itself in the production of new plant and replacements for itself and its associated blast furnaces; the dumping by American and German producers of Siemens bars first forced the producers of steel in the United Kingdom, who were supplying the Welsh tinplate industry with its staple raw material, to take a financial interest in tinplate firms. (Jones, Tinplate Industry, p. 162.)

by extending its original plant. Moreover a firm which seeks to make itself commercially "watertight," by producing all its own raw materials and no more may find this quite incompatible with the attainment of maximum technical efficiency in its different plants. This latter point merits amplification. Each of several processes which go to make the final product will have a certain scale of output at which production is most economical. Obviously, it will be only by chance that these different optimum sizes will provide a perfect fit. If a firm controlled two successive processes and the most efficient scale of output for the one were two units and the other three units, then, unless the market for its products was at least six it would either have to buy or sell raw materials, and sacrifice its commercial "balance," or run its "raw material" plant or its "finished product" plant on less than the most efficient scale. In short it would be forced to sacrifice some part of one or the other of the advantages claimed for integration. Generally speaking the chances that a perfect "fit" will be possible will depend upon the relation between the market which a concern has, and the output which is necessary to bring all the economies of large scale production in its plants; the greater the market and the smaller the plants the greater the probability being that the vertical firm will gain everything possible in both directions. But in industries where the most efficient plants are large, it is very likely that a firm will have to sacrifice something of commercial self-sufficiency or run its plants part time.

Faced with this dilemma, different industries have adopted different policies. In the case of a minor process absorbed by a larger, then usually the scale of output, even if it results in relative technical inefficiency, will subordinate itself to the size of the market provided by

the larger process. The railway companies in the United Kingdom construct their own locomotives but do not sell in the outside market. Carding is always carried on along with cotton spinning tho it is conceivable that separate and larger scale production would be cheaper. Presumably, when an iron works acquires a limestone quarry, it works it to satisfy its own requirements and not to produce the lowest cost per unit of extracted material. There is often a reluctance on the part of manufacturers to integrate if this forces upon them other branches of trade and manufacture. Tanning concerns rarely own forests to gain the extractions from the bark necessary in tanning, since this would make the lumber business incident to their tanning activities.[5] When the integrated processes are of equal or almost equal importance, it is not so easy to determine what will be the outcome of the rival influences. In some cases, as with the woollen section of the Yorkshire wool textile industry, the integrated firms seem to be almost self-sufficing and the purchase or sale of intermediate products small. In other industries the vertical organisation appears to enter into the market as buyer or seller. The British pig-iron manufacturers who control collieries in Great Britain supply large quantities of coal to the outside market.[6] Before the war, if steel works and tinplate factories were run by the same firm, the number of tinplate mills it was convenient to run under one control was not great enough to keep the most economical size of steel mill running full time.[7] The Meat Packers Trust started out to produce ice and salt for its own purpose but found it advisable to manufacture on a more

5. Federal Trade Commission, Report on Leather and Shoe Industry, 1919, p. 161.

6. Committee on Industry and Trade, Factors in Commercial Efficiency, p. 78.

7. Jones, Tinplate Industry, p. 170.

economical scale and sell its surplus.[8] Perhaps the outstanding example of integrated firms which do not, in practice, free themselves from the preoccupation and risk of entering into the market, either as buyers or sellers of raw materials, is provided by the concerns which both spin and weave in the Lancashire Cotton Industry. The following table shows the distribution in 1929 of a sample of 46 such "mixed" firms [9] according to the degree to which they entered the yarn market either as buyers or sellers. Altho half the firms sold less than 5

	Proportion of yarn consumed which was bought in outside market (Number of firms)							
Proportion of yarn produced which was sold in the outside market (Number of firms)	Per cent	0–5	6–10	11–20	21–30	31–40	40	Total
	0— 5	11	3	2	1	3	3	23
	6—10	3	..	..	1	..	..	4
	11—20	1	..	..	..	..	3	4
	21—30	3	..	..	1	..	..	4
	31—40	1	1	..	..	..	..	2
	40—	4	..	..	..	..	5	9
	Total	23	4	2	3	3	11	46

per cent of the yarn they produced and the same number bought less than five per cent of that they used in their weaving sheds, yet only 11 firms were balanced sufficiently to both sell less than five per cent of their product and buy less than five per cent of their needs. The conception of a mixed firm as a fairly watertight unit conveniently insulated against the trouble and risks of the market for raw materials is certainly, in this case, quite incorrect.

8. Federal Trade Commission, Report on the Meat Packing Industry, Part IV, p. 14.

9. This constitutes about one third of the total of such firms, and the sample was well spread over the different towns in the Lancashire cotton area.

Enough has been said here to show that, given certain simplified economic conditions, integration brings very few substantial advantages. This conclusion is supported by the available statistical investigation on this point which, altho it is not conclusive, appears to show that in iron and steel [1] the most highly integrated companies had a slightly lower rate of return of earnings to investment than those which were less integrated, and that in the petroleum industry [2] there was the same absence of correlation between rates of return and degree of integration.

When, therefore, the problem is considered from a static point of view and in relation to competitive conditions it is probably quite true that "there is little justification for the view that there is any general tendency to vertical integration throughout industry proper." [3] But in the light of the actual growth, noted above, of integration in many industries such a conclusion has little validity when applied to those basic economic conditions — such as a rapidly expanding market, a continually developing industrial technique and the prevalence of powerful monopolistic interests — which have obtained in American industry in the past two decades.

III

As soon as monopoly influences dominate the market, integration takes on a new importance. If a manufacturer finds his supply of raw materials or his outlet to the market threatened by a monopolist, he will seek to

1. Federal Trade Commission, Wartime Profits and Costs of the Steel Industry, p. 32.
2. Federal Trade Commission, Petroleum Prices, Profits and Competition, 1928, p. 293.
3. Lavington, op. cit.

safeguard himself by expansion even tho this costs him something in the way of efficiency. This appears to be one reason why the iron and steel companies in United Kingdom are rapidly acquiring control of coal and limestone.[4] Coöperative selling societies are being formed in many of the staple crops of U. S. A., one purpose of which is to break through the established control of the present middlemen.[5] The integration, again, may find its reason in offensive and not defensive tactics. It may seek to fortify its monopoly position in one process by controlling another. Thus the Aluminum Company of America, controlling the whole of the output of crude aluminum, has moved into the production of utensils, and appears to treat its competitors there, many of whom are dependent upon the company for their pig aluminum, in a very cavalier fashion.[6] Similarly, once the railroads in the anthracite coalfields of the United States had fortified their control of the means of transport by acquiring dominant interests as coal-mine owners, they were able to make very favourable bargains with the independent producers.[7] Generally, however, integration is less an offensive than a defensive weapon, for if a monopolist fully controls one link in the chain of production he can often dictate conditions on either side of him without acquiring interests there. The Standard Oil Company before 1911 rarely troubled to own oil fields, since the control which it exercised over refineries made it able to dictate the prices at which it

4. Committee on Industry and Trade, Further Factors in Industrial and Commercial Efficiency, p. 88.

5. The monopoly control exercised by middlemen here is usually indirect. In cotton marketing, it has often been alleged, the fact that the local merchant helps in the early financing of the grower and holds liens on the growing crop gives him a very strong hold over the price at which he purchases.

6. See Federal Trade Commission, Report on House Furnishings Industry, vol. iii, chap. 4.

7. See United States *v.* Reading Co., 226 U. S. 324.

would receive crude.[8] And the old American Tobacco Company was able to exert the same long-distance influence on the prices of leaf tobacco.

It is not so easy to ascertain the influence which fluctuations in the pace of industry have upon integration, or determine to what degree the vertical firm is more stable than the horizontal, for there appear to be forces poised against each other. Neither do the first-hand studies which have been made to compare the relative stability of the two forms of organisation help greatly. A survey which was made in 1927–28 among the gray-iron foundries of the United States shows that fluctuations in output were as frequent and as large among the integrated firms which made castings for their own use as among those which produced for sale.[9] On the other hand, the Federal Trade Commission's report on petroleum shows that the more highly integrated concerns showed smaller fluctuations in rates of return on investment during the years 1922–26 than the less integrated.[1] General reasoning makes certain generalisations possible, however. In the first place a period of depression is inimical to the integration of purely manufacturing processes. If integration during periods of prosperity enables "two profits to be made instead of one" it is equally true that ruinous competition results in the vertical firm making two losses instead of one. Thus of a sample of 46 mixed firms in the Lancashire cotton industry 39 declared that, on the whole, the integration of processes had been a disadvantage since 1920, as yarn could usually be bought in

8. Federal Trade Commission, Petroleum — Prices, Profits and Competition, 1928, p. 64.

9. Survey of Gray-Iron Foundries, Dept. of Commerce, Domestic Commerce Report, No. 29; cf. Bezanson and Gray, Trends in Foundry Production.

1. Federal Trade Commission, Petroleum — Prices and Profits, 1928, p. 293.

the market at a price lower than its full cost of production. And integration makes a concern less flexible, less capable of turning to newer and more profitable methods. In the cotton industries of both Lancashire and the United States the "mixed" firms have been less willing to take up the weaving of rayon, since they have had to face the problem of their overhead in spinning. A vertical firm cannot take advantage of cut-throat competition in an anterior industry as can a horizontal. One reason why the Ford Company retained its old model so long was that its highly integrated organisation was mobilised for the task of making model T, whilst other manufacturers were able to throw a great deal of the cost of constant changes in models and the necessary cost of readaptation of machinery upon the manufacturers of parts who had become so numerous that cut-throat competition had developed among them.[2]

Secondly, whilst a period of depression tends to disintegrate manufacturing processes, it appears to increase the cohesion between manufacture and marketing. Where the search for markets becomes frantic, the manufacturer cannot afford to leave his merchanting to common agencies; he must safeguard his own outlets. The period of excessive production and low prices in the petroleum industry after 1920 is one explanation of the movement of the various Standard Oil units towards independent integration.[3] In the Yorkshire woollen industry the slump of 1920 led many producers to engage in their own marketing to a greater degree. Further, where the merchanting side of the industry is quite separate from the manufacturing, it is possible for the merchant to exercise monopoly action which will en-

2. E. Flugge, "Integration in the Automobile Industry," Journal of Political Economy, April, 1929.

3. J. Ise, The United States Oil Policy, p. 235.

tirely rob the manufacturer of any beneficial results of the sacrifice of overheads which he makes. When the volume of trade declines, the merchant is tempted to meet this reduction in volume by an increase in the margin to which he works. A 20 per cent decline in the volume of trade upon which he was formerly making a 5 per cent margin can be entirely offset if he can increase his margin to 6¼ per cent, and his monopolistic position, arising from entrenchment by tradition and experience in specific markets, sometimes makes this possible. But where the manufacturer notices that his overhead costs are being absorbed elsewhere and are not going wholly to force down the final selling price, he naturally agitates for a more equitable bearing of loss and strives to come to some sort of working arrangement with the merchant. This appears to be one type of change which the depression in the Lancashire cotton industry is causing.

Thirdly, a period of prosperity and expanding profits will tend to stimulate integration. Boom conditions will often weaken caution and lead to uneconomic expansion both in degree and direction. More important still is the internal pressure which accumulated reserves exert in a concern which is flourishing and growing rapidly. In some American industries the habit of limiting declared dividends and reinvesting a large part of the net annual income has become almost morbid. A study of 2971 corporations in the United States between 1920–26 shows that of a total surplus available of $8,500,000,000 only 34 per cent was distributed in cash dividends.[4] The Meat Trust largely grew from within, for of a total of $504,000,000, earned up to 1919, $341,000,000 was left in the business.[5] The Bureau of Corpor-

4. Federal Trade Commission, Stock Dividends, p. 11.

5. Federal Trade Commission, Report on the Meat Packing Industry, 1919, Part V, p. 18.

ations reported that between 1901–10 the United States Steel Corporation made additional net investment of $505,000,000 of which $435,000,000 was virtually provided from savings.[6] This plethora of capital is likely to drive concerns on to indiscriminating expansion and the profitableness of their main lines then may enable them to support their mistakes almost indefinitely. The highly integrated Meat Packing Trust made lower profits on the whole of its operations than the smaller independent meat packers, and it has been suggested that this was due to the very low rate of profits earned by the trust on its integrated products such as canned foods, outside meat products, general groceries, packers machinery and chemicals. The General Electric Company has always had very large investments outside its immediate manufacturing activities, and in 1926 these outside investments amounted to $142,000,000 as against $223,000,000 invested in its own business. But the rate of earnings on the outside investments[7] has usually been well below that in its own business and from 1912–26 the former was only about one quarter of the latter.[8] It appears, therefore, that an expanding market will both stimulate integration and keep it in existence even when it proves uneconomic. There is further evidence that integration is usually bound up with growth and success in the fact that the integrated firm usually appears to be larger than the horizontal. If the integrated concern were more difficult to manage and organise than the horizontal, one would expect, other things being equal, that it would also be smaller,

6. Report on the Steel Industry, Part I, p. 49.

7. These consist partly of Government Bonds or Stocks and Bonds of other companies, but principally of advances to subsidiary and affiliated companies.

8. Federal Trade Commission, Supply of Electrical Equipment, 1928, p. 63.

the intensive problems of management being offset by a narrowing of the extensive. This does not seem to be the case. Thorpe,[9] using Census material, has shown that for groups of establishments operated from central offices the "simple" groups[1] have, on the whole, a smaller number of establishments than the "complex" groups. And special studies of industries where the two types of firms are engaged in much the same processes suggest the same conclusion.[2] The limitation of such statistical work, however, is that it cannot determine whether the vertical firm is large because it is integrated, or integrated because it is large and successful.

Fourthly, any changes in an industry, whether in the technique of production or the conditions of the market, will tend to rob vertical integration of some of its advantages. Technical changes often deprive integration of its independence of the raw material market. Twenty years ago the amount of scrap iron used in the making of steel was small and an integrated concern could largely supply itself with its own primary raw material from its own blast furnaces. Changes in technique have made it possible to use scrap iron on a much larger scale; in 1927 in the United States the scrap consumed represented 44 per cent of the steel produced and, since no concern has enough of its own, the vertical firms are forced to go outside for this raw material. Similarly a "mixed" cotton firm might have gained perfect commercial balance ten years ago, but the growth of the use of rayon yarn has made it dependent upon outside sources of supply.

9. United States Bureau of the Census, Monograph 3, 1924, p. 172 *et seq.*

1. "Simple" groups are those in which all the establishments are engaged in the same operation, "complex" groups those in which the different establishments are engaged in different activities.

2. Thus in the gray-iron foundries the firm using its own castings is larger than the firm which produces for sale (Survey of Gray-Iron Foundries, op. cit.).

Rapid changes in demand will also make more difficult and unlikely the coincidence of commercial balance and technical fit mentioned earlier in this paper. If the plants in two processes A and B have been balanced, a sudden increase in demand for the final product might be met in both by running the plants longer or working them at a greater speed. But it would be only by chance that the capacity for extra output would be the same for the two processes. The overwhelming likelihood is that differences in technique would result in differences of capacity for temporarily increased output. In such cases the vertical organisation would have to decide whether it would retain surplus capacity in one of its processes in order to meet the "peak-load" of the other, in which case it would normally sell this surplus in the open market, or whether it would ignore the peak-load problem, in which case fluctuations in demand would involve its entrance into the market as a buyer of intermediate products. In such cases the chance that the vertical firm may have to go into the market either to buy or sell intermediate products will become greater if the relation which overhead costs bear to the total cost of production varies between one process and another. Assume two integrated processes A and B, in the first of which overheads are 50 per cent of total cost and in the latter 10 per cent. Ignore the cost of the original raw material and assume that the two processes have the same cost. Then if process A could not find any market for its product save in supplying it to process B, i. e. if it could not sell in the outside market, the integrated firm would continue to sell the final product up to the point where it received 70 per cent of the total cost of production of the final product (i. e. 50 per cent of the cost of the first process plus 90 per cent of the cost of the second). If price fell below this mark, both plants

would be closed down completely. But what is more probable is that the product of process A could be sold in the outside market. If, under these circumstances, the price of the final product had fallen below 70 per cent of total cost of production whilst the market price of process A remained at something above 50 per cent of total cost, then it would pay the integrated firm to close down process B and continue working process A to cater for the outside market.[3]

IV

To summarize: Under dynamic industrial conditions an integrated concern is exposed to a number of forces the resultant of which it is not possible to determine. Conclusions as to the probable future trend of integrations must, in consequence, be tentative and conditional. Apart from technical influences, however, the normal advantages which the vertical firm possesses appear to be small. It is not necessarily more stable in the face of market fluctuations than the horizontal firm; the gains from a "balanced" plant are limited and precarious; it is tied to more markets and touches the industrial system at more points where a slight disturbance of equilibrium will demand immediate and costly readjustments. Integration appears to be bound up primarily with monopoly, in both an active and a passive sense, and with the rapid industrial growth that comes with an expanding market.

3. Whether such a position would arise would depend upon the relative elasticities of demand of the products of the two processes and whether the price changes had affected the whole industry or just a few firms. Thus if a new invention which very much reduced the price of steel were adopted by all but a few integrated firms, and the demand for steel were highly inelastic, neither the demand for pig iron nor its price would change very much and it would probably pay these firms to close their steel works and sell pig iron in the open market.

In the light of the foregoing analysis, however, it is possible to conclude that the divergence of views mentioned in the early part of this article is more apparent than real and less an indication of fundamental differences of opinion than of the diverse ways in which integration reacts to differences in the nature of the surrounding economy. In particular the apparent conflict in views of English and American writers is revealed as the natural outcome of the study of those facts which lie nearest to their hands. English industries, facing a stabilized or dwindling market and buying the bulk of their raw materials from abroad in markets which are world-wide, are not likely to experience strong impulses towards integration. In the United States the rapid growth of industry; the prosperity which accompanies this; the search for new outlets for capital investment; the habit of corporate saving; and the dominant control which a few large companies now exercise over such vital raw materials as anthracite, oil, bauxite, iron ore and copper — all these are sufficient to explain the present integration and the general anticipation that such a movement will continue in the future.

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HAS THE NATIVE POPULATION OF NEW ENGLAND BEEN DYING OUT?

SUMMARY

Introduction, 2. — Previous discussions, 2. — Fertility rates, 7. — Lack of adequate data makes it impossible to reach precise conclusions of New England dying out, 9. — But such data as are available support the contention that the natives of New England are not maintaining their numbers, 14. — Conclusions, 18.

It has frequently been alleged that the native stock of New England was dying out in the latter half of the nineteenth and in the early part of the twentieth century. Usually this assertion was coupled with a further allegation that foreign-born women, because of their greater fertility, would eventually inundate New England with immigrant stock and thus gradually replace the native element descended from the colonial stock of 1790.

In this paper I propose to review briefly the assertions of native decadence and the data offered as evidence. I shall then present the available data on comparative native and foreign fertility, and in the light of these data, examine, in so far as possible, the validity of the contention that the native stock of New England has been dying out through failure to reproduce itself.

I

The contentions that the native population was dying out were usually based upon examination of the birth rates of certain restricted elements of the population of New England, such as the population of Boston, the

population represented in genealogical records, or the selected population attending the colleges and universities of New England. The declaration that native women have fewer children than foreign women was usually based either upon the birth returns or upon the returns of the state censuses of Massachusetts and Rhode Island.

As early as 1850, according to Dr. H. R. Storer,[1] Jesse Chickering concluded that "the native element [in] the population of Massachusetts is stationary or decreasing."[2] Dr. Nathan Allen[3] cites Chickering to the effect that "the most important result derived from this view is the fact that the whole increase from the excess of births over deaths for these two years (1849–50) has been among the foreign population." Storer largely ascribed this failure to reproduce to the practice of abortion.[4]

Edward Cliborn in 1856 argued that because of unsatisfactory climatic conditions and because of occasional pestilential visitations Europeans could not survive in America. As evidence he cited "the non-increase of the numbers of people representing the old settlers in New York, Maryland, and especially the families who with Penn colonized Pennsylvania."[5]

The compiler of the Massachusetts Registration Report for 1869[6] declared that in Massachusetts the Eng-

1. See "On the Decrease of the Rate of Increase of Population now Obtaining in Europe and America," American Journal of Science and Arts, xciii (March, 1867), 141 ff.

2. Storer cites Chickering's "Comparative View of the Population of Boston" (1850), City Document No. 60.

3. See "Changes in Population," Harper's Magazine, xxxviii (1868), 388.

4. Op. cit., pp. 148–155; see also M. S. Iseman, Race Suicide, (New York, 1912), pp. 136–138.

5. See "The Tendency of European Races to Become Extinct in the United States," Report of the 26th Meeting of the British Association for the Advancement of Science (1856), p. 136.

6. Page 15.

lish stock is, "in spite of the very considerable English immigration of the past ten years, likely to be at no very distant day out-numbered by the Irish, the Germans, and the French Canadians."

In 1868, Dr. Nathan Allen, of Lowell, Massachusetts, published the first of a number of articles, in which he called attention to the decline in the fertility of the natives. He cited genealogical records to show that in the seventeenth century families in Massachusetts numbered eight to ten children, whereas the average number per family was only three about 1868. He gave as the causes of this decline physical degeneration in woman, foeticide, and the practice of contraception. He pointed out that the foreign birth rate was much higher than that of the natives and concluded that in Massachusetts "it will be difficult to find a margin between the birth-rate and death-rate among the strictly American sufficiently large to show a great increase of population, especially when it is considered that, as a general rule, two-fifths of all children born die before reaching adult life." [7]

Allen's contention that the decline in fertility was caused by physical degeneration was denied subsequently by General Francis Walker, who maintained that the natives cut down their birth rate in order to escape the intensified industrial competition and the

7. See "Changes in Population," Harper's Magazine, xxviii (1868), 388. See also "The Law of Human Increase," Quarterly Journal of Psychological Medicine (April, 1868); "Lessons on Population suggested by Grecian and Roman History," Congregational Quarterly (October, 1871); "The New England Family," The New Englander (March, 1882); "The Law of Human Increase," Popular Science Monthly (November, 1882). Allen's three pamphlets, "Population: its Laws of Increase," "Physical Degeneracy," and "The Law of Human Increase," are partly summarized in Nature, iv (1871), 62–63. That the fear of race decadence was frequently expressed immediately following the Civil War is indicated by references thereto in the Popular Science Monthly, lxiii (1903), 275, and lxv (1904), 51.

shock to their sensibilities caused by the heavy influx of foreigners.[8]

Thus far no satisfactory quantitative data have been presented by the writers quoted.[9] This remained to be done by Dr. R. R. Kuczynski, who called attention to European interest in the low fertility of Massachusetts [1] and who summarized some of the pertinent data included in the Massachusetts State Censuses and in the annual state registration reports.[2] His main conclusions were formulated as follows: [3]

I. At the three censuses of 1885, 1890, and 1895 the proportion of native adult men living in wedlock was three-fifths, that of the foreign born, two-thirds; the proportion of the native adult women living in wedlock was six-thirteenths, that of foreign born seven-thirteenths. These differences are even greater for both sexes in the reproductive period of life (1895). At the census of 1885 the number of women who were married or had been married without ever having borne a child was one-fifth among the natives and two-fifteenths among the foreign-born. The proportion was but one-ninth among the women born in Germany, French Canada, and Ireland. The average number of children born to every foreign-born married woman was two-thirds higher than for the natives. It was four-sevenths higher than for the natives among the German women, six-sevenths higher than among the Irish women, and more than twice as high among the women born in French Canada. The average

8. See Discussions in Economics and Statistics, i, 44; ii, 424–425. Walker's theory is criticized by E. A. Goldenweiser, The American Journal of Sociology, xviii (1912–13), 342–351. See also H. P. Fairchild, Immigration (1925), chap. 11.

9. Data showing that the fertility of the foreign-born exceeded that of the natives were secured in the Massachusetts State Census of 1875, 1885 and 1905, and in the Rhode Island State Census of 1885 and 1905. Data for Massachusetts are also presented by S. W. Abbott in "Vital Statistics of Massachusetts, A Forty Years' Summary," Twenty-eighth Annual Report of the Massachusetts State Board of Health, pp. 761–829.

1. See Arsène Dumont, "Essai Sur la Natalité en Massachusetts," Journal de la Societé Statistique de Paris, 1897, pp. 332–353, 385–395; 1898, pp. 64–69.

2. "The Fecundity of the Native and Foreign-born Population in Massachusetts," Quarterly Journal of Economics, xvi (1901–02), 1–36, 141–186.

3. Op. cit., pp. 184–186.

number of children living for every married woman was three-fifths higher among the foreign-born than among the natives. It was more than one-half higher for the women born in Germany, two-thirds higher for the Irish women, and nearly twice as high for the French-Canadians. All these differences between the natives and foreign-born are especially large in the reproductive age of life. The fact that the proportion of single among the adult natives was more than two-fifths, and among the foreign-born less than one-third, while it was not much less than one-half for the natives of Massachusetts, only a quarter for the natives of the other New England states, and only two-elevenths for the women born in Germany, makes the total adult women natives in Massachusetts even less prolific, while the natives of the other New England states approach more nearly the average of the adult women, and the prolificness of the adult German women is still greater.

II. In the quindecennial period from 1883 to 1897 the special marriage-rate of the foreign-born adult men is one-ninth, that of the women three-tenths higher than that of the adult natives. The special marriage-rate (1887–89) of the adult is smallest in the case of the natives of New England and the Irish. While it is rather high for the native males born outside of New England, it is very low for all native women. The refined marriage-rate of the not married adult foreign-born men exceeds that of the natives by three-tenths; that of the foreign-born women exceeds that of the native women by one-half. These differences are still greater in the reproductive age groups (1887–89). In the quindecennial period from 1883 to 1897 the special birth-rate of the adult foreign-born women is more than twice as high for the foreign-born than for the natives. It is especially low for the natives of New England, and still lower for those born outside of Massachusetts than for those born in Massachusetts (1887–89). In the quindecennial period the refined birth-rate of the foreign-born married women is four-fifths higher than that of the natives. The refined birth-rate of the foreign-born married women of child-bearing age is seven-tenths higher than that of the natives (1893–97). In the quindecennial period from 1888 to 1897 the general death-rate of the natives is nearly one-fifth higher than that of the foreign-born. The mortality of the natives calculated by age groups would probably be less for the natives than for the foreign-born.

With respect to the question as to whether or not the native population was holding its own, Kuczynski concluded: [4]

4. Op. cit., p. 184.

But as the tables of fecundity of Berlin show that, with an annual special birth-rate of ten for every one hundred women in child-bearing age in 1891–95, the births were one-ninth behind the number necessary to keep the population of Berlin stationary, it is probable that the native population of Massachusetts with a special birth-rate of 6.3 births for 100 adult women in child-bearing age, and a mortality of the female sex not correspondingly lower than that of Berlin, cannot only not hold its own, but is dying out at a considerable pace.

Kuczynski later remarked that "the exclusion of a large part of the immigrants might cost the United States its place among the world powers." [5] In an article published in 1907 [6] he presented fertility rates for Massachusetts and a number of European countries; only the rates for France and Ireland were as low as the rates for Massachusetts.

In 1903 F. A. Bushee presented figures for Boston [7] which indicated that the newly arrived foreign stocks had the highest birth rate and that in 1889–90 the native Americans with birth and death rates of 16.4 and 17.2 respectively "are increasing by propagation very little if at all."

Newsholme and Stevenson, on the basis of corrected birth rates for Boston, Providence, and Rhode Island concluded that "the condition of voluntary prevention of childbearing in the native population has gone far beyond that reached by Paris.[8]

Since early in the twentieth century numerous studies have been made of the fertility of college graduates in

5. "Die Einwanderungspolitik und die Bevölkerungsfrage der Vereinigten Staaten Von Amerika," Volkswirtschaftliche Zeitfragen, Heft 194 (Berlin, 1902), 35.

6. See "Zur Statistik der Fruchtbarkpit," Jahrbücher fur National Ökonomie und Statistik, xc (December, 1907), 230.

7. See "Ethnic Factors in the Population of Boston," Publications of the American Economic Association, iv (May, 1902), 46, 49.

8. "The Decline of Human Fertility in the United Kingdom and the Countries as shown by Corrected Birth Rates," Journal of the Royal Statistical Society, lxix (March, 1906), 87.

the colleges and universities of New England. These studies show a gradual decline in the number of children per married college graduate, especially after 1850, and indicate that the college graduates were not replacing themselves.[9] The first of these studies appeared about the time that President Roosevelt and President Eliot of Harvard in 1903 expressed a fear of "race suicide."

Genealogical studies[1] by Engelmann, F. S. Crum, C. E. Jones, and others corroborated the earlier studies made by Allen. F. S. Crum, who studied twenty-two genealogies of families that originally settled in New England or the middle Atlantic States, found the average number of children per wife previous to 1700 to be 7.37; 1700–49, 6.83; 1750–99, 6.43; 1800–49, 4.94; 1850–69, 3.47; 1870–79, 2.77.[2] C. E. Jones studied

9. See, for example, E. L. Thorndike, "The Decrease in the Size of American Families," Popular Science Monthly, lxiii (1903), 64–70; G. S. Hall, and T. L. Smith, "Marriage and Fecundity," The Pedagogical Seminary (September, 1903); F. W. Nicolson, "Family Records of Graduates of Wesleyan University," Science, xxxvi (July 19, 1912); N. S. Nearing, "Education and Fecundity," Quarterly Publications of the American Statistical Association, xiv (1914–15), 156–174; R. S. Sprague, "Education and Race Suicide," Journal of Heredity (April, 1915); J. C. Phillips, "A Study of the Birthrate in Harvard and Yale Graduates," Harvard Graduates Magazine (September, 1916); Mary Van Kleeck, "Census of College Women," Journal of the Association of Collegiate Alumnae (May, 1918); L. I. Dublin, "The Significance of the Declining Birth Rate," Science, xlvii (1918), 201–210; M. and Gibson E. Newcomer, "Vital Statistics from Vassar College," American Journal of Sociology (January, 1924); Warren S. Thompson, "Natural Increase of Population," chap. 3, especially pp. 33–37, 48–49, in L. I. Dublin's Population Problems in the United States and Canada (1925); E. A. Ross and R. E. Baker, "Changes in the Size of American Families in One Generation," University of Wisconsin Studies in the Social Sciences and History, No. 10 (1924).

1. G. J. Engelmann, "Education Not the Cause of Race Decline," Popular Science Monthly, lxiii (1903), 172–184; F. S. Crum, "The Decadence of the Native American Stock," Quarterly Publications of the American Statistical Association, xiv (September, 1914); C. E. Jones, "A Genealogical Study of Population," Quarterly Publications of the American Statistical Association, xvi (1918–19), 201–219. A good bibliography is included in Jones' study. See also F. L. Hoffman, "The Decline in the Birth Rate," North American Review (May, 1909).

2. Op. cit., pp. 216, 218, 220–222.

3,876 couples and found the average number of children per couple during the period 1651–1700 to be 5.8; 1701–50, 5.9; 1751–1800, 6.1; 1801–50, 4.8; 1851–1900, 3.0.[3] It is doubtful whether either 2.77 or even 3.0 children[4] per married couple were adequate to replace both the married and unmarried persons within the portion of the population described in these genealogies.

Studies by Allyn Young and J. A. Hill indicated among other things that foreign females bore more children than native females, that native-born married women of foreign parentage bore a higher average number of children than did native-born married women of native parentage, and that the proportion of married women bearing children decreases more rapidly with age among the native than among foreign married women.[5] Walter Willcox in 1911 indicated that the decline in the proportion of children in the American population reflects a decline in birth rate since about 1810.[6]

The studies we have summarized indicate that (*a*) the fertility of native women was less than that of foreign women; and (*b*) that it is likely that some elements in the population of New England were hardly replacing themselves. These studies, however, do not disclose the relative magnitude of the fertility rates of native and foreign-born women in New England for an extended period of time; nor do they indicate whether the native

3. Op. cit., pp. 209–217.

4. L. I. Dublin estimated that, about 1918, 3.2 children per family were necessary to replace the population; see op. cit., Science (March, 1918), p. 208.

5. See Young, "The Birth-rate in New Hampshire," Quarterly Publications of the American Statistical Association, ix (1904–05), 263–291; Hill, "Comparative Fecundity of Women of Native and Foreign Parentage in the United States," *Ibid.*, xiii (1914), 590–597.

6. See "The Change in the Proportion of Children, etc.," Quarterly Publications of the American Statistical Association, xii (1910–11), 490–499.

population of given states or of New England was replacing itself after 1850. The remainder of this paper will be devoted to a solution of these two problems.

II

In this study I shall use fertility rates instead of crude birth rates for two reasons: (*a*) the use of fertility rates (number of children born per 1,000 women aged 15 to 49 years) eliminates the major differences in the respective age compositions of the native and of the foreign female populations; and, (*b*) the fertility rate may be made the basis of a crude means of determining whether or not a population is replacing itself.

TABLE I

THE AVERAGE ANNUAL NUMBER OF LIVING BIRTHS PER EACH 1000 FEMALES, AGED 15 TO 49

Years	Maine	New Hampshire	Vermont	Massachusetts	Rhode Island	Connecticut	New England
1848–50				95 [a]		76	
1851–55				99		77	
1856–60			82 [b]	102	86	91	
1861–65			69	88	77	79	
1866–70			86	91	85	85	
1871–75			84	95	85	89	
1876–80			82	84	82	82	
1881–85		63 [c]	77	86	79	86	
1886–90		65	77	88	88	82	
1891–95	82 [d]	75	81	94	87	86	88
1896–1900	81	78	84	93	91	84	88
1901–05	81	77	87	86	89	83	85
1906–10	87	83	87	90	88	89	89
1911–15	84	82	86	89	86	93	89
1916–20	86	84	87	87	89	100	90
1921–25	93	85	89	80	79	79	81

(*a*) 1849–1850. (*b*) 1857–1860. (*c*) 1882–1885. (*d*) 1892–1895.

In Table I is given the average annual number of children born per 1,000 women aged 15 to 49 years and living in New England. Tables II and III show the average annual number of children born respectively per 1,000 native and 1,000 foreign-born women aged 15 to 49 years.[7]

TABLE II

THE AVERAGE ANNUAL NUMBER OF BIRTHS PER EACH 1000 NATIVE FEMALES AGED 15 TO 49

Years		Maine	New Hampshire	Vermont	Massachusetts	Rhode Island	Connecticut	New England
1853–	55				78			
1856–	60				76	65 [a]		
1861–	65				62	56		
1866–	70				64	64		
1871–	75				67	67		
1876–	80				64	67	63 [b]	
1881–	85		53 [c]		66	63	65	
1886–	90		53		65	60	64	
1891–	95	70 [d]	57		65	61	64	65
1896–1900		69	57	76 [e]	63	61	60	64
1901–	05	69	57	76	58	59	57	64
1906–	10	75	62	77	60	59	55	62
1911–	15	75	65	77	60	59	56	62
1916–	20	79	71	80	64	65	65	67
1921–	25	86	80	84	68	67	64	71

(*a*) 1857–1860. (*b*) 1878–1880. (*c*) 1884–1885. (*d*) 1892–1895. (*e*) 1900.

The results given in Tables II and III can be summarized briefly. Immediately following the outbreak of the Civil War there was a sharp decline in native fertility in Massachusetts and Rhode Island. There is no

7. These rates are taken from the writer's "The Comparative Fecundity of the Native and of the Foreign-Born Women of New England," in the pamphlet series of the Brookings Institution, Washington, D. C. In this pamphlet the history and degree of completeness of birth registration in New England, the character of the data available, the conclusions derivable from the data, etc., are treated at length.

evidence whatsoever for a further decline in native fertility after 1870. In fact, after 1910, we find native fertility increasing somewhat. [8] The foreign-born women have always maintained a greater fertility than the native women. The foreign fertility, however, has declined markedly in the past fifty years. If the present trend continues, foreign fertility will approximate native fertility in magnitude.

III

Has the native stock of the respective New England states been replacing itself during the period under review? Accurate determination of an answer to this question requires: (*a*) the ages of the mothers giving birth to children, and (*b*) a life table for the females whose net

8. Part of this increase may be due to improved registration. The failure of the native fertility rates to decline is probably caused in part by the fact that the native population consists of an increasingly larger portion of natives with one or both parents foreign-born; for natives of foreign parentage on the average produce more children than natives of native parentage. (See J. A. Hill, op. cit.). In New England in 1870, 17 per cent of the natives had one or both parents foreign-born; in 1890, 22 per cent; in 1920, 42 per cent.

The decline in foreign fertility apparently is largely the result of an increased voluntary restriction of births rather than of changes in age or marital composition. Data for Rhode Island show that nine tenths of all births were to mothers aged 20 to 39 years. In New England in 1870, of each 100 foreign-born women aged from 15 to 49 years, 68 were from 20 to 39 years old; in 1900, 68 per cent; in 1910, 66 per cent; in 1920, 64 per cent. Official population estimates since 1920 do not reveal the age composition of the foreign population; however, assuming no migration, on the basis of the 1920 census returns and life table, about 58 per cent of the foreign females, of each 100 foreign-born females aged 15 to 49 years during the period 1921–25 about 58 ranged in age from 20 to 39 years. This increasingly unfavorable age composition is the result of the reduction in the annual influx of female immigrants; the average annual number of female immigrants to the United States was 346,357 in 1910–14; 96,760 in 1915–19; 239,221 in 1920–24.

The marital composition of the foreign female population of New England has become increasingly favorable to fertility. In 1890, of each 100 foreign-born women aged 15 to 44, 54 were married; in 1920, 72. (See pamphlet by writer, op. cit., for data on age and marital composition.)

reproductivity is being measured.[9] Neither class of information is available for New England, however. In only a few of the registration reports of New Hamp-

TABLE III

THE AVERAGE ANNUAL NUMBER OF BIRTHS PER EACH 1000 FOREIGN-BORN FEMALES, AGED 15 TO 49 *

Years	Maine	New Hampshire	Vermont	Massachusetts	Rhode Island	Connecticut	New England
1853–55				149			
1856–60				162	134		
1861–65				147	125		
1866–70				144	126		
1871–75				148	117		
1876–80				122	108	127	
1881–85		104		123	108	129	
1886–90		108		129	113	123	
1891–95	142	126		141	129	136	138
1896–1900	142	131	127	140	138	133	138
1901–05	137	127	147	131	134	135	133
1906–10	138	132	143	137	133	153	139
1911–15	122	121	131	134	125	161	137
1916–20	116	118	127	128	128	166	134
1921–25	121	98	119	103	128	108	102

* See footnotes under Table II.

shire and Rhode Island is the age of both native and foreign-born mothers given. For no state do we have such information on the respective mortality of native

9. See in this connection R. R. Kuczynski, The Balance of Births and Deaths, i, chap. 3. The future trend of natural increase as affected by the changing age composition of the population has also been treated by: E. Cannan, "The Probability of a Cessation of the Growth of Population in England and Wales during the Next Century," Economic Journal, v (1895), 505–515; L. I. Dublin and A. J. Lotka, "On the True Rate of Natural Increase," Journal of the American Statistical Association (September, 1925). See also "The Ultimate Population of the United States," Statistical Bulletin, Metropolitan Life Insurance Co. (January, 1930), pp. 12–13; P. K. Whelpton, "Population of the United States, 1925–1975," American Journal of Sociology (September, 1928).

and foreign-born females as is necessary to construct a life table for the native population of native parentage and for the population that is foreign-born or of immediate foreign extraction.

The death rate of the foreign population is higher than that of the native when differences in age composition are taken into consideration.[1] The life tables for the native population of either Massachusetts or the United States are rendered slightly less favorable to longevity by the fact that the specific death rates of native children have been increased by the inclusion of the deaths of native-born children of foreign parents. The specific death rates of natives of foreign parentage are somewhat greater than the specific death rates of natives of native parentage, altho lack of data makes impossible the calculation of the exact degree of difference. Hence,

1. See, for example, J. W. Glover, "United States Life Tables," United States Bureau of the Census, 1921, pp. 88–103; F. L. Hoffman, "The General Death-Rate of Large American Cities," Quarterly Publications of the American Statistical Association, x (1906–07), 18–19; L. I. Dublin, "Factors in American Mortality," American Economic Review, vi, no. 3 (1916); Niles Carpenter, "Immigrants and Their Children," United States Bureau of the Census (1927), pp. 197–210. For the period 1866–75 decedents in Massachusetts were classified according to nativity of parentage, but the population was not so classified in correlation with age. During this period there were 148.5 deaths under one year of age per 1,000 births to native women of native parentage and 172.2 per 1,000 women of foreign-born parentage. Allowing for the fact that some of these decedents of foreign parentage were born abroad, the foreign-born had an infant mortality rate of 169, 14 per cent greater than the native rate. In Providence, Rhode Island, the native infant mortality rate was generally below that of infants of foreign parentage (Providence Registration Report, 1914, p. 102). In Massachusetts the specific death rate of children aged 0–4 years and of foreign parentage was apparently between 20 and 24 per cent higher than that of children of native parentage. In the Rhode Island Census of 1895 (p. 346) data are given to show that but 46.6 per cent of the children born to American mothers who had borne 15 or more children by 1895 were surviving at the time of the census; of the foreign, 48 per cent were living. All the available data indicate that on the average a larger proportion of the children born to native mothers survive than of those born to foreign mothers. We can not measure survival power accurately, however, as the ages of the children surviving are lacking.

were only the specific death rates of natives of native parentage employed in the construction of a life table for natives, the average number of years of life indicated by the life table would be greater than the average derived from a table for natives which is based in part upon the specific death rate of natives of foreign parentage.

Since the life tables that are available are apparently not truly representative of the mortality of natives of native parentage, we shall have to employ either (*a*) the experience mortality tables of insurance companies or (*b*) the life tables of those European countries with the lowest mortality. The former are based upon too select a population.[2] Instead, so as not to overstate native mortality in New England, we shall select those European life tables which yield the greatest average length of life. We explain below the use made of these life tables.

The assumption that native mortality in New England was not less than that of those European countries with the most favorable mortality conditions is basic to the argument to be presented. If native mortality was actually less than that of selected European countries our argument that the natives did not always replace themselves loses force. If native mortality exceeded that of the selected European countries, our argument is strengthened. That our assumption is practically valid seems to follow from the fact that the average number of years spent in childbearing, according to life tables, was only 88 per cent as high for natives and foreign-born combined in Massachusetts about 1855 as in Sweden; the average in Massachusetts for 1901 and 1910 combined was only 93 per cent as high as that of Norway for the period 1901–10. To invalidate our assump-

2. See J. W. Glover, op. cit., pp. 224 ff., for the leading American experience tables.

tion, it would be necessary to show that general health conditions were better among the natives of New England than in Scandinavia.

Since, as we have shown, we lack the data necessary to measure accurately the degree to which the native population was replacing itself, we must employ such a method as the nature of the data permits. In order that a population shall not eventually decrease, each set of 1000 newly born females must in the course of time replace itself with 1000 female children. Assuming that there are 105 males to every 100 female births, 1000 newly born females, to replace themselves, must bear $2.05 \times 1000 = 2050$ male and female children. Hence, if 1000 females, only part of whom live to and through the childbearing period (fifteenth to fiftieth year), in the course of a lifetime bear 2250 children, they will have maintained a net reproduction rate of $2250 \div 2050 = 1.098$. That is, each female will have averaged .098 more births than was necessary to replace the population. If these 1000 newly born females bear only 1750 children they will have maintained a net reproduction rate of but $1750 \div 2050 = .859$; that is, only 85.9 per cent as many children would have been born as were required to prevent a decrease in population. In other words, with a net reproductivity of but .859, the population would be doomed to extinction, if the assumed mortality and natality continued.

The actual number of children that 1000 newly born females will eventually bear before they complete the childbearing period is obtained by multiplying the prevailing fertility rate by the average number of years these 1000 spend in childbearing. Thus if a sufficient number of the original 1000 females reach the age of 15 and live through part or all of the 35 years of childbearing period to give a total of 25,000 years spent in child-

bearing (25 years per female) and an average annual fertility rate of 90 is maintained, these 1000 newly born females ultimately bear $25 \times 90 = 2250$ male and female children. It is apparent, therefore, that the lower mortality is, the greater will be the average number of years spent in the childbearing period; and the greater the average number of years spent in the childbearing period, the smaller will be the fertility rate required to prevent a decrease in population. Reversing this proposition, the lower the actual fertility rate the greater will be the number of years that will have to be spent in childbearing provided a stationary population is to be maintained.[3]

If the number of years that actually are spent in childbearing is less than the number of years that, with a given fertility rate, need to be spent in childbearing to replace the existing population, the population will not be replacing itself. For example, let us assume that the average fertility rate is 60; then an average of 34.17 $\left(\frac{2050}{60}\right)$ years per woman will have to be spent in childbearing if the population is not to die out. But suppose that, according to the life table, an average of but 30 years is spent in the childbearing period; then but $\frac{30}{34.17}$, or .878,[4] as many persons would be born as needed to be born to replace the existing population; hence the existing population would be dying out.

In passing, it may be noted that in a population where

3. For instance, if the average fertility rate were 100, one thousand newly born females would need to spend an average of but 2050 divided by 20.5 years in childbearing = 100. If the fertility rate were 60, the number of years necessary would be increased to 2050 divided by 60 = 34.17.

4. In other words, 2050 (60×34.17) children must be born; but only 1800 (60×30) are born. Hence only $\frac{180}{205}$ children are born of the number necessary to maintain a stationary population.

the average length of life and the average number of years spent in childbearing are low, the fertility rate is likely to be higher than in a longer-lived population. The reason for this is the fact that in a short-lived population a larger percentage of the women of childbearing age is living in the age groups in which natural fecundity is greatest (15 to 40) than was true of New England where the age composition was almost constant from 1870 to 1920. So far as our argument is concerned, however, we merely use the observed fertility rate to determine the mortality condition necessary for the population to replace itself. This mortality condition, derivable from observed data, is then compared with the mortality condition that prevailed or was assumed to prevail. Accordingly, we shall compare these two mortality conditions with respect to the natives of New England.

To determine whether the native females were replacing themselves we need to know the average number of years they lived in the childbearing period. But, for reasons indicated above, this information is lacking. Apply now the assumption that mortality conditions among the native stock of New England were no better than those of the most favorable countries in Europe. This assumption, in the light of what we know of mortality conditions in New England, hardly involves an overstatement and may even involve an understatement of actual mortality among the natives of New England. In Norway mortality conditions were as favorable or more so than in any other country in Europe in the second half of the nineteenth century. The average number of years spent in childbearing during the period 1856–65 was 23.689; 1881–82, 24.181; 1901–10, 27.241.[5]

5. See E. Levasseur, La Population Francaise, ii, 335; Glover, op. cit., p. 210.

In Sweden, according to Kuczynski [6] the averages were as follows:

1816– 40	22.08
1841– 50	23.13
1851– 60	22.20
1861– 70	22.88
1871– 80	23.57
1881– 90	24.63
1891–1900	25.56
1901– 10	26.97
1911– 15	28.04
1916– 20	27.23
1921–	29.29

The averages for Norway and Sweden are appreciably higher than those obtaining in the entire population of Massachusetts. In 1855, the Massachusetts average was 19.568; in 1880, 21.734; in 1890, 21.806; in 1901, 24.488; in 1910, 26.347; in 1919–20, 27.732. The highest average for the entire population of an American state in 1919–20 was 28.976 for Wisconsin.[7]

The average number of years spent in the childbearing period by women in Norway will be utilized to calculate net reproduction rates for natives in each state. These are derived as follows. Take Massachusetts for the period 1856–60. According to Table II, the native females of Massachusetts had a fertility rate of 76 in

6. See Balance of Births and Deaths, i, 49. The Swedish average was greater than those of Denmark, England and Wales, and Germany.

7. The 1855 figure is based upon the life table computed by E. B. Elliott in 1855, on the basis of mortality returns for 166 Massachusetts towns (Sixteenth Registration Report of Massachusetts, 1857, p. 204). No distinction is made between male and female mortality. The figures for 1890, 1901 and 1910 are from J. W. Glover's life tables for Massachusetts females (United States Life Tables, 1890, 1901, 1910 and 1901–10, 1921, pp. 138 to 143). The figure for 1880 is from a table computed by J. S. Billings, white females of Massachusetts (Census of 1880, vol. xii, Part II, p. 791). The figures for 1919–20 are computed from E. Foudray, United States Abridged Life Tables, 1919–20, white females of Massachusetts and Wisconsin, pp. 18–19.

1856–60; hence 2050 ÷ 76 = 26.97, the number of years required to replace the population. However, in Norway during the period 1856–65, 1000 females ultimately spent an average of but 23.7 years in childbearing. Hence, since 26.97 years were required and but 23.7 were spent in childbearing, it follows that 1000 newly born females, under such mortality and natality conditions, would bear only 23.7 divided by 26.97 = 0.88 as many children as were needed to replace the population. The native population, on the basis of these figures, was dying out.

IV

In Table IV we give net reproduction rates for each state and for New England. These rates indicate that only in Maine, New Hampshire, and Vermont was the native population increasing at any time. If our assumed European life tables are representative, the population of New England was dying out prior to 1920.

This conclusion must be qualified in several ways, however. In the first place the method of measuring net reproduction employed by us is somewhat inaccurate.[8] Secondly not all births were registered in any New England state in the nineteenth century.[9]

8. Kuczynski used a method mathematically equivalent to ours for Sweden in 1871–80, 1891–1900 and 1901–10; he also used his refined method. By the cruder method he secured results 3.9, 1.8 and 2.3 per cent lower than those obtained by the refined method. See op. cit., pp. 29–31, 47–48. That is, the method we have used may tend slightly to understate the number of births and thus to make the derived net reproduction rate slightly lower than it would be if the nature of the data permitted the use of Kuczynski's refined method. However, as the age composition of the native population remained practically unchanged from 1870 to 1920, the results yielded by the method employed are substantially correct.

9. Some births have always escaped registration in New England, but the proportion has steadily declined. In proportion as births escape registration the fertility rates given in this paper are too low. Thus, if the fertility rate derived from the *registered* births is 80 when only 80 per

TABLE IV

Net Reproduction Rate of Native Women of New England

State	1856–60		1866–70		1876–80		1886–90		1896–00		1906–10		1916–20		1921–25	
	Years required[b]	N. R. R.[c]	Years required	N. R. R.	Years required	N. R. R.	Years required	N. R. R.	Years required	N. R. R.	Years required	N. R. R.	Years required	N. R. R.	Years required	N. R. R.
Maine									29.71	.88	27.33	1.00	25.95	1.10	23.84	1.26
New Hampshire							38.64	.65	35.96	.73	33.06	.82	28.90	.99	25.63	1.17
Vermont									26.97[e]	.97	26.62	1.02	25.63	1.12	24.40	1.23
Massachusetts	26.97	.88	32.03	.75	32.03	.78	31.54	.79	32.54	.87	34.17	.80	32.03	.89	30.15	1.00[f]
Rhode Island			32.03	.75	30.60	.79	34.17	.73	33.61	.78	34.75	.78	31.54	.91	30.60	1.00[f]
Connecticut					32.54[d]	.74	32.03	.78	34.17	.77	37.27	.73	31.54	.91	32.03	.94
New England									32.03	.82	33.06	.82	30.60	.94	28.90	1.04
Selected European Country[a]	23.70		23.94*		24.18		25.00*		26.26*		27.24*		28.62		30.00*	

a. Average number of years spent in childbearing in European country with most favorable mortality conditions. Asterisk (*) indicates this is an estimate.

b. Years of childbearing required to replace population with fertility rates given in Table II equals $\frac{2050}{\text{Fertility rate.}}$

c. Net reproduction rate (N. R. R.) is obtained by dividing (a) into (b).

d. 1878–80.

e. 1900 only.

f. Approximately stationary.

Nevertheless, it is doubtful whether the native population increased at all in most of the states; it is more likely that the population failed to replace itself. For instance, during the years 1866–70 in Massachusetts and Rhode Island, natives produced 26 per cent less children than were required to replace the native population. Assuming that our European life table was typical for these states at this time, is there evidence that, say, 20 per cent of the births escaped registration and that our method yields results 6 per cent too low? The answer seems to be in the negative.

It does not follow that because the natives were approximately replacing themselves in Massachusetts and Rhode Island in 1921–25 that they had always done so. If our estimate of population is correct for 1921–25, this apparent net reproduction rate of 1.00 may be the result largely of the assumed reduction in mortality. We incline to the view that by 1910 the descendants of the natives of Massachusetts, Rhode Island, and Connecticut of 1860 were probably slightly less in number than the number of natives living in these states at the time of the Civil War; at the best there was no appreciable increase. As to New Hampshire, even if the native population did not increase to the moderate extent

cent of the births occurring are registered, *true* fertility would be represented by the rate of 100 instead of by the given rate, 80. If the observed fertility rates are too low because of incomplete registration the net reproduction rates derived from these incomplete data are also too low.

It is impossible to measure accurately the percentage of births that escaped registration (see pamphlet, op. cit., for consideration of methods of measuring omitted births). In Massachusetts in 1858 one seventh were not registered. In 1919, according to the late William H. Davis, M.D., Chief Statistician for Vital Statistics, the following percentages of births were registered: Massachusetts, 100; Rhode Island, 99.3; Connecticut, 97.1; New Hampshire, 95; Vermont, 92.6; Maine, 90.9. The precise accuracy of these estimates is not ascertainable. It suffices to say that there is apparently no evidence to show that our conclusions are invalidated by the omission of some of the births from the registration records.

shown, it is unlikely that it decreased appreciably.[1] With respect to Maine and Vermont it seems probable, in the light of the births escaping registration in these states, that the native population was not decreasing.

We have no adequate data for the whole of New England in the immediate post-Civil War period. Crude calculating, however, indicates that the natives of that area were, at the most, slightly more than replacing themselves about 1870. In 1870 in New England there were 361,300 native children under five; this number may be fairly raised by 2 per cent to 368,500, to offset possible errors in age returns. Assuming that each 7825[2] children represents an original 10,000 during the five years ending May 30, 1870, the average annual number of births was 94,180. During these five years the average annual number of native females, 15 to 49 years old, living in New England, was 688,000; the foreign average was 222,000, of whom one half lived in Massachusetts. If each 1000 foreign females, aged 15 to 49, averaged 150 births,[3] the 222,000 bore 33,300 children per year. The native women bore 60,880 (94,180 − 33,300), maintaining a fertility rate of 88.[4]

With a fertility rate of 88, 23.3 $\left(\frac{2050}{88}\right)$ years of child-

1. In New Hampshire many births escaped registration; net reproduction was nearly 1.0 in 1916–20.

2. According to Kiaer's life table for Norway, 1856–65, of 10,000 children born during a five-year period, 8589 would survive; Elliott's table for Massachusetts in 1855 gives only 7825 survivors. Logically, if Elliott's value for survivors is employed we ought to employ the average number of years spent in childbearing according to Elliott's table, namely 19.568, instead of 23.689 as given by Kiaer.

3. In Massachusetts the foreign fertility rate was 144 in 1866–70; this rate we increase to 150 to allow for births that escaped registration (in 1858 one seventh escaped registration: Massachusetts Registration Report for 1858, p. v.). We have assumed only a 4 per cent deficiency in foreign registration. However, in Rhode Island, the observed foreign rate was but 126; this rate would be increased to nearly 150 by including omitted births.

4. The observed fertility rate of the natives in Massachusetts in 1866–70 was 64; in Rhode Island, 64.

bearing are required to replace. The Norway average, 1856–65 was 23.7; that for part of Massachusetts, 1855, 19.6. Hence, if our assumptions are correct, the natives were little more than replacing themselves, if that.[5]

The nature of our treatment indicates that we cannot state with great accuracy the extent to which the native population was dying out in parts of New England. However, when allowance is made for deficiencies in the data, it becomes apparent that in Massachusetts, Rhode Island, and Connecticut in the second half of the nineteenth century the native females had a fertility rate apparently too low to increase the population, under the existing conditions of mortality. Mortality improved somewhat in the course of this period, whereas fertility remained approximately constant, until in the present century the then-native[6] population was replacing itself. When allowance is made for deficiencies in the data of Maine and Vermont, it seems apparent that the native population was increasing somewhat. The native population of New Hampshire, bearing in mind deficiencies in registration, seems to have been approximately at a standstill.

5. In 1870 there lived in New England 702,000 native and 250,000 foreign women aged 15 to 49. On June 1, 1870, census enumerators counted in New England 74,911 native children under one year of age. According to E. B. Elliot (Census of 1870, ii, 531) because of overstatement of age and omissions each 2854 children, reported as under one year, represents 3212 children. According to the life table for Massachusetts, 1855, of each 10,000 children born in the course of a year 8914 would be living at the close of the year. Hence, about 94,500 ($74{,}911 \times \frac{3212}{2854} \times \frac{10{,}000}{8914}$) children were born during the year ending June 1, 1870. If the foreign females maintained a fertility rate of 150, they bore $50 \times 150 = 37{,}500$ children; the natives then bore $94{,}500 - 37{,}500 = 57{,}000$ children. With the resulting native rate of 81 ($\frac{57{,}000}{702}$), 25.3 years of childbearing would be required to replace the population unless fertility increased or mortality decreased.

6. The "native" population, it must be remembered, included many of foreign parentage by 1900.

In the last analysis the conclusions reached in this study depend for validity upon the validity of the hypotheses and methods employed. In the light of available evidence they appear valid. With this qualification in mind, our conclusions, based on the study as a whole, are these:

(1) Previous studies indicate that in the latter part of the nineteenth century certain elements of the native population of New England were dying out.

(2) During the period for which we have data, the fertility of native women has been lower than that of foreign women.

(3) There is no evidence tending to show a decrease in the fertility of native women in the last fifty years.

(4) The fertility of foreign-born women has decreased greatly in the course of the last fifty years.

(5) Until the present century, and possibly not in that century, the native population of Massachusetts, Rhode Island, and Connecticut was not replacing itself. That of New Hampshire was probably at a standstill, while that of Maine and Vermont showed some increase.

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MOORE'S SYNTHETIC ECONOMICS

The Climax of a System

An attempt to appraise Henry Ludwell Moore's newest book, Synthetic Economics, makes one wonder how the earlier book, Forecasting the Yield and Price of Cotton, seemed to economists when it appeared.[1] The elaborate mathematical and statistical mechanism of correlation coefficients and regression lines, parabolic trends and forecasting equations, must have seemed as foreign and unintelligible a decade ago as the differential formulae and solid masses of simultaneous equations of Synthetic Economics will seem to many economists now.

Yet, more than any other American economist, Moore is responsible for the introduction of the methods of statistical analysis — not mere statistical description — into economic usage, and for the growing liaison between economic theory and the known facts of concrete economic happenings. He applied statistical methods in Laws of Wages as early as 1911, and next developed his well-known theories of economic cycles. The use of definite mathematical curves, which, while they might approximately reproduce, could not *explain* recurrent fluctuations in time, injected an element of rigidity and unreality into his treatment of periodicities, and helped

1. The series of books, of which this is the latest, comprises the following:
Laws of Wages, 1911.
Economic Cycles: their Law and Cause, 1914.
Forecasting the Yield and the Price of Cotton, 1917.
Generating Economic Cycles, 1923.
Synthetic Economics, 1929.

to prevent their general acceptance. But in Economic Cycles: Their Law and Cause we can also find the first stages of that application of statistical methods to price analysis, which is — at least until this most recent book — Moore's most significant contribution. In his chapters in Economic Cycles on The Law of Demand he shows for the first time the possibility of tying together economic theory and the facts of economic life, of clothing the theoretical supply and demand generalizations with the concrete facts of actual occurrences. These methods are developed much more fully and completely in Forecasting the Yield and Price of Cotton. In fact, a careful rereading of that work impresses one with the broad and comprehensive scope of his treatment. It is almost a Magna Carta for the hundreds of statistical "price analysis" studies which have been made in subsequent years, by such younger men as Holbrook Working, Hugh Killough, Bradford Smith, Henry Schultz, Edmund Daggit, Fred Waugh, and Louis Bean. Almost every phase that has since been developed was either worked through or suggested in the cotton book — the use of regression equations as a basis for prediction; measuring the influence of prices upon subsequent acreage; measuring the accuracy of condition figures as forecasters of yield; forecasting yields from rainfall and other weather factors, by the aid of multiple correlation; measuring the influence of changes in supply upon price, and so forecasting prices as soon as supply could be judged. Even the possibility of the simultaneous determination of the influence of changes in price level upon the price of a particular commodity is developed, and worked out in one of the examples. Of the three broad phases of price analysis studies now generally recognized, (1) the influence of supplies on prices, (2) the influence of prices on subsequent production, and (3) the influence

of prices on consumption and storage, the first two were explicitly developed in that book. In addition, two vexing problems which are still unsettled, the treatment of time elements ("secular changes") and the treatment of changing general price levels, were recognized and attacked.

But even tho in the two earlier books Moore integrated statistical analysis and economic theory, he did not succeed in doing away with the empirical character of the statistical treatment. In the more than a decade between The Price of Cotton and Synthetic Economics, Moore has refined his tools of analysis so that now each mathematical equation has a definite significance in terms of economic theory. In his own words, he can now so choose "the types of economic equations that their parameters shall reveal the nature of important economic coefficients."[2] His formulation of the nature of the elasticity of demand as the point of departure in the statistical treatment of demand curves is an excellent illustration of this point.[3]

The development of a more philosophical approach to price analysis is, however, only a small part of Synthetic Economics. Its sweep is far broader. A decade ago, Moore was already interested in Walras' simultaneous equations "for the purpose of completely surveying the interrelated factors in the problems of exchange, production, and distribution."[4] In the same way that his earlier developments clothed the theories of supply and demand with the actualities of concrete facts, Synthetic Economics attempts to make Walras' formulation con-

2. Synthetic Economics, p. 98, note 3.

3. This treatment was first developed in Moore's article in the Journal of the American Stat. Assn., "Elasticity of Demand and Flexibility of Prices," March, 1922, pp. 8–19. It is repeated, in somewhat condensed form, in chap. 3 of Synthetic Economics.

4. Forecasting the Yield and the Price of Cotton, p. 172.

crete — to show the possibility of bringing all economic relationships into measured and known equations, and thereby of explaining the detailed interrelations of all actual economic events. It is a breath-taking scheme. Not only, says Moore, can all types of economic interrelations now be measured, but they can be measured under the conditions which do exist — partial monopolies, economic friction, and all — without having to restrict the analysis by unreal assumptions as to "pure competition" or "static conditions." Furthermore, he suggests a method of verifying the applicability of the hypothesis; for the results, independently secured as to prices and productivities, must again be consistent when inserted in the "productivity formula."

Before attempting to evaluate or criticize Moore's proposals, we must have them clearly before us. I shall therefore first review briefly the essential elements of his scheme, neglecting for the time being the many interesting digressions which represent ramifications of the central theme.

Synthetic economics, as defined by Moore, includes (1) "the use of simultaneous equations to express the *consensus* of exchange, production, capitalization, and distribution"; (2) the extension of this treatment into dynamics by treating all variables as functions of time; and (3) giving all the equations "concrete, statistical forms," by which he means both stating them in such a way that the actual values of all the constants can be empirically determined from the facts of actual occurrences, and then (presumably) proceeding to make such determinations. Under these conditions, he claims, "Synthetic Economics is both deductive and inductive; dynamic, positive, and concrete."

The essential elements in the formulation are embraced in the chapters on The Law of Demand, The Law

of Supply, and Moving Equilibria. Certain implications of the theory are developed in the further chapter on "Economic Oscillations," but these are independent of the scheme as a whole, and subsidiary to it.

Moore uses the term "law," we must remember, in the same sense as the older physicists used it, to designate a definite mathematical statement of relationships. His treatment of demand follows the traditional mathematical economics of Cournot and Marshall, quantity-taken being stated as a function of price-demanded. He places even greater emphasis on the elasticity of demand, however, than did Marshall, for that coefficient is the key to Moore's whole mathematical formulation of the problem.

In stating the precise mathematical form of the demand function, various assumptions may be made. Moore bases the equation upon assumptions as to the nature of the elasticity of demand. We may assume, he says, either:

(*a*) that the elasticity of demand is the same at all points along the curve — that is, that the elasticity does not change with price; or,

(*b*) that the elasticity of demand changes by a constant increment (or decrement) as price changes, so that the elasticity changes continuously and progressively along the curve; or,

(*c*) that the elasticity of demand changes at a changing rate as price changes. This last may permit the elasticity to become first higher and higher as price is increased until a maximum is reached and then become lower and lower again; or *vice versa*.

These three different assumptions may be expressed mathematically by representing the elasticity of demand (*a*) as a constant; (*b*) as a linear function of price; or (*c*) as a parabolic function of price. Still more elaborate assumptions might be represented by parabolae of still higher orders.

With three different assumptions as to elasticity of

demand, there are three corresponding "typical equations" of the demand curve, expressing the relations in such a way that the elasticity of demand itself may be readily determined. For the simplest assumption, the corresponding demand curve is

$$\log D = B + \beta \log p$$

(D = quantity demanded, p = price, B and β are constants to be determined from the data).

Here η, the elasticity of demand, is given directly by β, that is $\eta = \beta$.

In addition to the three forms for the equation of the demand curve, Moore discusses the case where quantity taken is considered as the independent variable, and price as the dependent variable. The demand curve then has the general form

$$p = f(D)$$

(price is a function of the quantity taken).

To meet the needs of this statement, he introduces a new coefficient, the "flexibility of price," which is simply the reciprocal of η, the elasticity of demand. By making the same three different assumptions as to the nature of this coefficient as he makes for the elasticity of demand, he obtains three more "typical equations" for the demand curve, or six in all. This six-fold presentation of every relation considered is carried all through the rest of the book, and adds very much to the oppressive visual weight of the presentation. It will not be mentioned again in the present summary; the reader must remember, however, that these six alternative developments are considered at every possible point.

The demand equation (in terms of a single variable) is then made dynamic by stating both price and quantity

as a ratio to trend. That is, using a bar over the symbol to represent the ordinate of trend,

$$\frac{D}{\bar{D}} = F\left(\frac{p}{\bar{p}}\right) \tag{1}$$

(the quantity demanded, in per cent of trend, is a function of price, also in per cent of trend). Skipping the statistical demonstration of the actual fitting of such a demand curve, the next step is the introduction of the simultaneous treatment of *all* prices. This involves stating the demand for any one commodity as a function of the prices of all other commodities. The method may be illustrated by assuming a universe of three commodities: A, gold; B, wheat; C, cotton, and two services: L, services of persons; K, services of capital. Moore, of course, uses a much fuller and more comprehensive notation. Expressing prices in terms of gold, the demand curves for the two other commodities are

$$\left.\begin{aligned} D_b &= F_b\,(p_b, p_c, p_l, p_k \\ D_c &= F_c\,(p_b, p_c, p_l, p_k \end{aligned}\right\} \tag{2}$$

(The quantity of each commodity demanded is a function of the prices of all commodities and services.) Each variable may also be stated as a ratio to its trend, as in (1).

In the same way that mathematical equations were obtained for the single-variable demand curve, they may be obtained for these multi-variable curves, by making appropriate assumptions as to the *partial elasticities* of *demand*; that is, the elasticity of demand with respect to each one of the component prices, the other prices meanwhile remaining unchanged. By making the same assumptions with respect to these partial elasticities of demand as explained before, mathematical forms for equation (2) of varying complexity are obtained. (Moore

also makes the implicit assumption that the partial elasticity of demand with respect to any one price remains the same, no matter at what level or combination of levels the remaining prices are held constant.)

The "Law of Supply" is then developed by an exactly parallel approach, giving the coefficient of elasticity of supply the same critical significance as the elasticity of demand was given before. "Supply functions" for many variables are likewise developed, to express the hypothesis that the supply of any one commodity in a given statistical period is influenced by the prices of *all* commodities.

In similar manner, production equations are developed to state the condition that the quantity of each commodity or service produced is a function of the quantity of other commodities or services employed or used up in its production; and cost and price equations to state the condition that the trend price of each unit of product will equal the number of units of other goods and services consumed in its production, each multiplied by its corresponding trend price per unit.

Moore thus obtains four groups of equations, which restate the Walrasian equations of demand, of supply, of production, and of cost and price, in such a way that the constants may be actually evaluated from the data of a dynamic universe. In the simplest form, one of each of these four groups of equations is as follows (in terms of the same limited group of commodities and services represented in previous equations).

Demand equations:

$$\frac{D_b}{\overline{D}_b} = \left(\frac{p_b}{\overline{p}_b}\right)^{\beta_{bb}} \left(\frac{p_c}{\overline{p}_c}\right)^{\beta_{bc}} \left(\frac{p_l}{\overline{p}_l}\right)^{\beta_{bl}} \left(\frac{p_k}{\overline{p}_k}\right)^{\beta_{bk}} \qquad [I]$$

Supply equations:

$$\frac{S_k}{\overline{S}_k} = \left(\frac{p_b}{\overline{p}_b}\right)^{\gamma_{kb}} \left(\frac{p_c}{\overline{p}_c}\right)^{\gamma_{kc}} \left(\frac{p_l}{\overline{p}_l}\right)^{\gamma_{kl}} \left(\frac{p_k}{\overline{p}_k}\right)^{\gamma_{kk}} \qquad [II]$$

Production equations:

$$\epsilon_{ak}\frac{\bar{p}_a}{\bar{p}_k}D_a + \epsilon_{bk}\frac{\bar{p}_b}{\bar{p}_k}D_b + \epsilon_{ck}\frac{\bar{p}_c}{\bar{p}_k}D_c + \epsilon_{lk}\frac{\bar{p}_l}{\bar{p}_k}D_l = S_k \quad \text{[III]}$$

and finally, equations of cost and price:

$$\epsilon_{bl}\frac{\bar{p}_b}{\bar{p}_l}p_l + \epsilon_{bk}\frac{\bar{p}_b}{\bar{p}_k}p_k + \ldots + \quad = p_b \quad \text{[IV]}$$

Moore states "The four groups of equations . . . , like Walras' equations, determine a general equilibrium, but the equilibrium with which they are concerned is real and not hypothetic, is moving and not static. It is a moving equilibrium about the lines of trend."

There are, in addition, ingenious equations for introducing the rate of interest into the system of equations; arriving at the supply of credit as a function of all prices (including interest); arriving at the valuation of capital goods; and expressing the relation between savings and cost of capital goods. These add four more groups of equations to the four previously enumerated, which serve to state the general moving equilibrium. They will not be presented here in detail; what has been said covers the most basic features of Moore's proposal. Introducing interest rates and the other new terms into the entire system of equations, Moore then arrives at a "second approximation" to "a moving general equilibrium"; which "takes its course along the lines of the general trends of the variables. The general trends . . . of the demand functions, the supply functions, and the production functions, are the trends obtained from equations fitted . . . by the methods of least squares. . . . In case of the other variables the general trends are the values . . . as they are determined by the *ensemble* of the equations describing the general equilibrium."

As a final check, the values obtained by the construction and solution of the whole set of equations may be

used to test the productivity theory of distribution. Thus each equation of type (IV) may be restated in the following manner:

$$\left(\epsilon_{bl}\frac{\bar{p}_b}{\bar{p}_l}\right)\frac{p_l}{p_b}+\left(\epsilon_{bk}\frac{\bar{p}_b}{\bar{p}_k}\right)\frac{p_k}{p_b}+\quad\ldots\ldots=1$$

Then multiplying through by Q_b it becomes

$$\left(\epsilon_{bl}\frac{\bar{p}_b}{\bar{p}_l}\right)\frac{p_l}{p_b}Q_b+\left(\epsilon_{bk}\frac{\bar{p}_b}{\bar{p}_k}\right)\frac{p_k}{p_b}Q_b+\ldots=Q_b$$

Since the quantities in parentheses are the coefficients of production, showing how many units of L and K are necessary per unit of B, the last equation becomes

$$L_b\frac{p_l}{p_b}+K_b\frac{p_k}{p_b}+\ldots=Q_b$$

(L_b merely means the number of units of L used in producing Q units of B.) "Now when the economic system is in a state of equilibrium, the productivity theory is supposed to hold, and we have shown that, when equilibrium is reached, equilibrium prices are trend prices and equilibrium products are trend products. By substituting equilibria values in the above equation" (trend values) it becomes

$$\bar{L}_b\frac{\bar{p}_l}{\bar{p}_b}+\bar{K}_b\frac{\bar{p}_k}{\bar{p}_b}+\ldots\ldots=\bar{Q}_b$$

which may be restated

$$\bar{L}_b\frac{\partial Q_b}{\partial L_b}+\bar{K}_b\frac{\partial Q_b}{\partial K_b}\qquad=\bar{Q}_b \tag{3}$$

(The trend production of B may be exactly distributed to L and K, on the basis of their marginal contributions to its production.)

This "is proof, in a form which may be statistically tested, of the proposition that in a state of equilibrium

the product of industry is divided according to the productivity formula."

Moore then goes on to discuss "economic oscillations," either "about particular equilibria," or "about general equilibria." In addition to an "index number of oscillations," he follows through the whole set of equations the probable repercussions of fluctuations in various individual factors, in the light of the inter-relations indicated. In this way he is enabled to arrive at judgments as to the extent to which fluctuations in various individual factors may be reflected in other elements of the economic system, and so enabled to appraise the extent of the "basis of truth" underlying each of a number of current theories of business fluctuations, and to point out that no one factor can be held alone responsible. "The theory of moving equilibria gives a synthetic view of all sources of oscillations, and it affords a technique, not only for appraising the relative importance of perturbations in the various sensitive spots of the industrial organism, but also for giving the oscillatory resultant of simultaneous perturbations in any number of sensitive spots."

Even tho this summary of Moore's statement is not brief, it is much simplified from his treatment, being condensed to roughly a dozen equations, while he uses 164 in his book. Many of his single equations fill nearly half a page apiece. But it is even more difficult to appraise his proposal as a complete synthesis of fact and theory than it is to sketch it in outline form.

Moore's proposal to represent all economic relationships in a single set of simultaneous equations, statistically derived, may be discussed from three points of view: (1) the theoretical adequacy of the system; (2) the statistical possibilities of carrying it into effect; and (3)

the economic and statistical adequacy of the particular techniques which Moore proposes.

To begin with the theoretical adequacy of the system. It is quite evident that Walras' formulation, even with Moore's modifications, regards all economic activity as following mechanistic laws, in much the same sense that physicists, a half century ago, were explaining physical relations in completely rigid terms. In the physical sciences, much of the rigidity of these earlier explanations has been discarded under the newer ideas of relativity and the nature of matter. In economic relations, such non-mechanical elements as human custom and inertia, the dead hand of the past, the rise and decline of institutions, organized or unorganized; political, social, and religious interventions; and many other non-calculable elements, may all be present to modify or prevent the things from happening that should happen "according to theory." It is exceedingly doubtful if *all* economic activity could ever be comprehended within the range of fixed mathematical relations. Certain limited phases, within which the assumed conditions can be or have been demonstrated to hold true with a fair degree of approximation, may be subjected to measured explanation; but not the totality.

Even were the universality of the system admitted, the specific validity of individual assumptions would be subject to grave question. Thus in the equations of type (I) and (II), Moore states demand and supply as functions of all prices, with varying degrees of complexity of statement for the partial elasticity of demand (or supply) with respect to individual prices. Yet, however complex his treatment becomes, it assumes that the partial elasticity with respect to the price of B never varies except with respect to differences in that price alone. But this assumes that no matter at what level the prices of the other items C, D, etc., are held constant, the

influence of a given change in the price of B on the coefficient of elasticity will be the same. The assumption is by no means justifiable. It might readily be true that the elasticity of demand with respect to changes in the price of B would be quite different when C and D were high in price from what it would be when C and D were low in price. Extending the argument, it might be quite possible that not only is the *total* demand for any one commodity a function of the prices for all commodities, but that the partial elasticity of demand with respect to changes in any individual price is likewise a function of the prices of *all* commodities, rather than of a single one. To express this mathematically for all n prices and n elasticities, in terms of definite equations, would so enlarge equations (I) or (II) as to make it practically impossible to determine their constants "concretely."

A similar serious objection can be raised against the adequacy of the mathematical formulation of the production and distribution equations (III), and (3). These equations make the assumption that the marginal change in production with a marginal change in the quantity of one productive factor is not affected by the differences in the quantities of other productive factors in use at the same time. For example, the additional crop which might be expected to be produced on a given farm by adding one more hour of labor would be the same on this assumption, no matter whether that additional labor was combined with two horses or with a 40-horsepower tractor — the *additional* effect of the *additional* labor is assumed to be the same. The assumption entirely ignores the organic nature of production processes, and the composite nature of the units in which modern production technique abounds.[5]

5. It may be demonstrated mathematically that the type of equation necessary to satisfy the productivity theory of distribution is one particular type of mathematical function (a homogeneous function); and that

One further point along the same line. Moore repeatedly remarks that the number of equations is equal to the number of unknown values, and hence can be solved by simultaneous solution. That holds true, however, only when each unknown is a simple constant of the first degree. If, with the increasing complexity of the inter-relations suggested in the preceding paragraphs, the number of constants necessary to express each unknown were greatly increased, many more equations would have to be provided.

To turn to the second question, the concrete one. Is it statistically possible to determine all the various demand, supply, and production constants from concrete data? This is a much less abstract question than those previously considered. Early in the book Moore points out that heretofore approximate conclusions have been obtained by regarding the demand for a commodity as a simple function of its price, on the theory of the negligibility of indirect effects. Now he proposes to go clear to the other extreme — "to represent demand and supply not as functions of a single price but as functions of all prices." The actual evaluation, he states, can be readily performed by multiple correlation, considering all prices simultaneously.

Unfortunately, even the warmest advocates of multiple correlation must admit its weaknesses as well as its strength. The reliability of the results as to the size of any particular constant obtained by multiple correlation decreases as the number of variables concerned in-

the productivity function may very readily be of other types. This particular point has been well developed in the paper by Moore's student, Henry Schultz, on "Marginal Productivity and the General Pricing Process," in the Jour. Political Econ., October, 1929 (particularly pages 543–545). Incidentally, in many respects Schultz's paper makes a clearer statement of Moore's formulations of the Walrasian equations than does Moore's book itself, and also carries the analysis of the coefficients of production much further.

creases. The minimum number of independent observations which should be available to evaluate the influence of a number of variables *must* be in excess of the number of variables, and for accurate results, must be well in excess.

Thus if the demand for cotton could be expressed as a function of only 100 prices in all (and that would not be sufficient to enumerate the commodities directly or indirectly related to cotton, let alone other commodities, still more remote), it would require say 150 independent observations to establish the relations. With an annual crop such as cotton, annual observations would be required to even approach the desired degree of independence in the observations. The determination of the elasticity of demand for cotton would thus have to be based on the entire history of cotton production in this country, even back to well before the cotton gin was invented!

Obviously such an extreme range of analysis would have no significance. Only on the assumption of some substantial degree of uniformity in economic conditions during the period considered can correlation analysis be applied to time series at all. It would seem that a middle position (which Moore himself developed in some of his earlier work) still offers the most reasonable basis of approach in statistical price studies. The demand for a particular commodity may be stated as a function of the price for it and of the prices of those commodities with which it is most closely in competition. In this way some of the indirect forces may be allowed for, yet the analysis be kept within workable—and statistically significant—bounds. Thus in studying prices of lamb, the prices of chicken, veal, beef, and pork have also been found to be significant, and their effect has been approximately determined simultaneously, just as Moore suggests.

Finally we have to consider the economic and statistical adequacy of the techniques which Moore himself offers for the concrete solution of the problem. The greatest shortcoming which the economic statistician will feel is in the treatment of the time element. Simply fitting empirical trends to series of data, and expressing them in percentages of trend, is not an adequate treatment of the dynamic element in economic activity. In such a statistical process, the influences of *all* factors which change progressively in time are eliminated, and this may include economic factors just as well as others. Moore himself appears to recognize this in his statement (p. 152) "these empirical laws may be obtained in a way that takes cognizance of the changes in their form with the passing of time." While the development of this suggestion (which he does not attempt) would partly meet the difficulty, so long as trends were fitted directly to the original series of data, the conclusions would remain open to question. The recent development of statistical price analysis has very largely been away from the initial elimination of trends, and instead regards time as one factor in the situation along with many others. It seeks to understand and interpret the changes that occur in time, rather than to throw them all into the unexplained blanket category of "secular change."

As a matter of fact, price analysis studies during the last decade have developed much more along the lines of Moore's earlier suggestions than along the lines proposed in Synthetic Economics. They have included more and more relevant series of prices and production, treated by multiple correlation; they have improved the methods of considering time elements simultaneously with other elements; and they have developed the preliminary hypothetical analysis of specific situations beyond that

covered, in detail, in earlier economic theory. To a considerable extent these studies have been successful in actually forecasting future economic developments. To some extent at least, the hope which Moore expresses for synthetic economics, that of "introducing into economic life rational forecasting and enlightened control," may be said to have already been secured by the developments from his own earlier work.

Despite these possible criticisms, it cannot be said that Synthetic Economics will always remain a dream. Economic statisticians are really getting well started only now on the lines which Moore first worked out fifteen years ago. It may be that once again his proposals are too far ahead, too startlingly different from what has been done, for us to grasp them or visualize them in their entirety. Certainly various small sectors can be worked out, if only by a further development of the methods of gradual approximation which have been used in the past. As more and more elements become clear, some few of the equations as a whole may be established and tried out. Even tho the entire range of economic activity is not covered, it may be possible to solve a set of the simultaneous equations for the major commodities, or at least for those whose markets most nearly fulfill the assumptions of fairly free competition and definite economic readjustments. The picture as a whole may never be completely worked out; but having it before us as a challenge, a goal, or a daring prophecy, may lead us to think in broader terms, and to look further ahead than we would otherwise have dared to do.

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REVIEW

FRANKFURTER AND GREEN'S LABOR INJUNCTION [1]

THE book on The Labor Injunction by Professor Frankfurter and Mr. Green is chiefly on the legal aspects of the matter. It begins with a striking account of the development of the law, of the American cases, of the extraordinary extent to which the American courts have carried the practice. The methods of procedure and the actual doings in the cases — often not indicated at all in the recorded opinions — are brought out in most interesting fashion. The proposals for legislation and the enacted legislation are considered, special attention being given to the Clayton Act (the Federal Trade Commission Act), one section of which purported to restrict the use of the labor injunction in federal courts, but was worded in such ambiguous language that the United States Supreme Court interpreted it (whether rightly or wrongly) as almost without effect on the existing law. Most consideration of all is given to the bill now before Congress, the so-called Shipstead Bill, which again attempts to restrict the practice, and is described and analyzed with much minuteness. Indeed the book is in some sort a plea for the enactment of that measure. Throughout we have not only citation of all the cases, such as is incumbent on a book addressed to the legal profession, but illuminating extracts from the opinions and from the non-legal literature. So far as a layman can judge, the book is a most workmanlike performance.

The legal situation is remarkable. The essentials can be stated, I think, briefly and simply. The peculiar state of American law rests on a matter of procedure, and that again

1. The Labor Injunction, by Felix Frankfurter and Nathan Green. New York, 1930: Macmillan. Pp. 343.

rests on the ancient and persisting distinction in Anglo-Saxon law between the common law and equity. A "labor injunction," like any other issued by a court sitting in equity, is supposed to rest on substantive law already established. *Aequitas sequitur legem;* such is the maxim. A judge sitting in equity enjoins — forbids — a person from doing things only if that person is infringing on the established rights of others when he does them. And, further, the judge enjoins only when the aggrieved person has, for the time being at least, no adequate remedy through the ordinary processes of the law. The usual statement is that an injunction will issue if the aggrieved (the plaintiff) is in danger of suffering "irremediable damage" — such as cannot be made good by an eventual suit at law for damages. The labor injunction cases arise where the leaders or members of a trade union or like organization threaten to do something in the course of a strike or boycott (1) which they have no right to undertake, and (2) which is likely to cause damage irremediable unless they are stopped at once.

The application of this procedure in cases of dispute and conflict between employers and workmen is, in its present form, a development peculiar to American (United States) jurisprudence. Nothing of the kind exists in England (some early moves that way have long ceased). It cannot be said to have been entered on lightly in the United States; yet, in this matter as in others, the systematized and far-reaching results to which it has gradually led were hardly dreamed of at the outset. No one can read these pages without being convinced that the judges who have developed it were "taking sides" — were not merely applying established legal principles, but were led, by repugnance to labor unions and a desire to prevent employers from being troubled, to a juridical situation virtually new. The most remarkable illustration of their bias is the application to these matters of the Fourteenth Amendment, and their interpretation of "property" as protected by the amendment. Employers are supposed to have a "property right," under which they may not be molested by epithets, "verbal castigation," and the like; and a state law restricting the use of injunctions may not deprive them of this

"property" (page 177). The way in which the Fourteenth Amendment, passed for the protection of the liberated slaves of the South, has been interpreted is a strange chapter in judicial history. Its application to state legislation on the "utilities" is familiar; but in other directions also, even more remote from what people had in mind after the Civil War, its scope has been made extraordinarily wide, and nowhere more so than in its utilization for preventing the states from checking the labor injunction. Our authors make it clear, moreover, that, quite apart from the stretching of the legal principle, the judges have often been loose to the extreme in accepting perfunctory allegations and affidavits as sufficient basis for issuing their injunctions. The record, as disclosed in these pages, not only explains the hot wrath which the labor leaders and their sympathizers pour on the judges but may be fairly said to justify it.

There is more to be said, however. Admitting the lengths to which the courts have gone, we can still see a consistency in the principles of social policy they have followed.

The basis of the procedure of the courts, as I see it, at bottom is not so much hostility to unions or favor to employers — these play their large part, no doubt — as a desire to make the existing law of torts and contracts work with some certainty and despatch. It is a move for escaping from the looseness, the uncertainty, the clumsiness of the machinery of the civil law and even more of the criminal law. While the law relating to labor troubles is in many respects unsettled, there are plenty of things which are clear. A strike may be coupled, and is supposed commonly to be coupled, with doings that are quite beyond the pale of the unquestioned law, and quite beyond the pale of unquestioned public policy. There must not be physical violence, destruction of property, disorder and riot. "Peaceful picketing" is within the pale; but what is peaceful? A shrewd observer has remarked there is no such thing as peaceful picketing. The judges in many of the cases here recorded evidently feel so. To be sure, the strikers are liable in suits for damages, and may be liable to criminal prosecution. But a suit for damages is a precarious remedy,

and a criminal prosecution is a weary and uncertain business. An injunction is not supposed to forbid, in so many words, the committing of a crime; that would be stultifying the law. But it can be granted against a wrongful act, even tho that be also a crime; and then the wheels move quickly and decisively. He who violates the injunction is thrown into jail because he is "in contempt of court." There is no waiting for the long and cumbrous machinery of indictment by grand jury, trial by jury, guilty or not guilty or disagreement — heaven knows what outcome or when. An able, astute, and fearless lawyer, the late Secretary Olney (then attorney-general), saw in the injunction a short-cut by which the law could be brought to bear with effect and despatch in a situation where there was supposed to be danger of calamity, even breakdown of the whole social fabric. The course of action to which he gave an enormous impetus in the Debs case has grown into a settled practice, and thereby the law in effect has been revolutionized.

I have used phrases familiar in the discussion of these matters — breakdown of the social fabric, unquestioned public policy, the existing law and its underlying principles. Of course there are most difficult questions behind these phrases. Can it be said there are settled principles of law — really settled — which "equity" is supposed merely to follow, and to enforce more effectively? How should those principles be formulated, and how far have the courts, in this new procedure, departed from accepted principles or found the right principles?

Here we come to the economic and social philosophy which, according to our authors, is applicable to the situation. Their view is that the courts have departed from the fundamental principle on which the system of private property rests.

> Our economic system is founded upon the doctrine of free competition. . . . ; large aggregations of capital are not inconsistent with the doctrine of free competition, but are inevitable and socially desirable; the individual workers must combine in order to achieve the possibilities of free competition with concentrated capital. . . . The right of combination by workers is in itself a corollary to the dogma of free competition. [Pages 202, 203.]

This seems to me ill-reasoned, or at all events ill-stated. Free competition does not mean, at least as the economists use the term, what is here laid down. True, the economists are themselves not always clear and consistent in their use of it. But the essential concept is that of competition as something that equalizes. It means a bidding against other people who are offering the same thing and must accept the same price. Free competition of workmen equalizes wages. It may equalize in either of two senses: in the sense that all get the same time-wages or effort-wages; or in the sense that all get the same efficiency-wages. In either case competition acts primarily as an equalizing force. The same holds of capital; under free competition each unit of capital gets the same return. Whatever extension we give to the notion of competition as an equalizing force, the conception remains different from what our authors have in mind when they speak of free competition between labor and capital.

Meaning by free competition what the economists usually mean, and viewing the matter quite coolly and objectively, I cannot reach any other conclusion than that free competition is promoted rather than restricted by the labor injunction; and on the other hand, is restricted rather than promoted by the strike. A strike is not a mere quitting of work because the men do not like the terms offered. It does not mean that they simply refuse to accept the employer's offer, leaving it to others to accept if they will.[2] The strikers want to hold the job on terms satisfactory to themselves, and want no one else to bid against them at all. Free competition, however, in the sense in which I would use the term, means that anyone willing to do the work on other terms may have it. To put the thing bluntly, it means that the scab is to have his free chance. This, I imagine, is precisely what the substantive law (the common law) assumes to be desirable and tries to bring about. The procedure in equity and the labor injunction are merely a newly developed legal mechanism for maintaining unfettered bidding.

2. I will not repeat what I have said on the strike and all that hangs thereby in my Principles of Economics, chaps. 56, 57.

I say this not by way of defending or justifying the courts, or of saying that strikes and unions and collective bargaining are all bad, but in order to indicate that the fundamental questions cannot be disposed of by arguing that there has been departure from an ancient and accepted principle. What our authors apparently believe desirable is smooth-working bilateral monopoly, or bilateral restriction; that is, a situation where one set of persons bargains as a body with another set, each set in an equally strong position and each untroubled by outsiders. As a matter of "straight economics," not only is this different from free competition, but the outcome is indeterminate. As a matter of social doctrine, the outcome is hardly more determinate; it is not clear that a result will ensue such as conforms to any accepted principle of social justice.

There are other turns of phrase in the book which come closer to the social question involved. We read of "the right of collective bargaining or complete unionization . . . as a means of strengthening resources," and that "the individual workers must combine in order thereby to achieve the possibilities of free competition with concentrated capital." If "free competition" in this passage had been replaced by "effective bargaining," that question would have been indicated. The plain fact is that the large-scale employer has the individual workman much at his mercy. He has a bargaining advantage, even if he is not trustified—the analog to complete unionization; he has it more if he is trustified. Too often he uses the advantage in ways which our sense of justice or sympathy condemns as harsh and inequitable. True it may be contended that in the long run this advantage does not greatly or permanently affect wages. Perhaps there remains after all enough effectiveness of competition between employers, enough elasticity in the supply of capital and management, to bring about eventually the same adjustment as if they had not the dominance in the individual case. Perhaps something of the sort can be said of unionization; it also may be rather a means of smoothing and regulating competition between workmen than of removing it, and may leave the final out-

come the same as if there were no unions. I am not sure about all this long-run reasoning, but in any case am quite sure that, as things stand, and with regard to the discernible results, the men may well be wary of giving up their own organizations. The plain facts of the dealings between men and "masters" show that for long periods and for many individuals, the unorganized workers get less than is supposed to be brought about by the long-run tendencies and the "economic laws." One cannot read these pages without feeling that our courts have gone far in minimizing the bargaining strength which the unions bring to the men, and that they have been much actuated by a sympathy or prejudice in favor of the property-owning classes. The bad sides of unions are obvious enough, and quite enough descanted on. Nor is it easy to say what kind of organization is good now, what the future will bring, which way we should move. Given the existing situation, the unions and the strikes make the workmen stronger.

Every one is affected in these matters by his prepossessions, by his sympathy for the under dog or his lack of it. Most of the judges have little sympathy. Judges like Holmes and Brandeis, and teachers like Frankfurter, have a good deal of it. I suspect that at bottom this, rather than any theory of free competition, explains their attitude on the labor injunction.

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NOTES AND DISCUSSIONS

FURTHER THOUGHTS ON DEPRECIATION AND THE RATE BASE

THE decision of the Supreme Court in the Baltimore Railways case (The United Railways and Electric Company of Baltimore *vs.* West et al., Public Service Commission of Maryland, decided on January 6, 1930) adds a new chapter to the legal history of the depreciation question in relation to rate regulation. The decision of the question immediately at issue was one of great technical interest; the implications of the decision are perhaps of even greater importance, tho that importance may not be so immediately apparent. It is proposed to deal briefly with both points.

Depreciation enters into rate cases in two ways. Depreciation attributable to a particular period is a charge against the earnings of that period, and the accumulated depreciation at any given date operates to reduce the rate base (that is, the value of the property employed in rendering service) at that date. In this instance, it was the first only, the charge against earnings, that was in question; the rate base was practically agreed, and the treatment of depreciation in fixing that base was not before the Court.[1]

The problem of fixing a depreciation allowance to be recouped out of charges for service is, in part, one of deciding

1. In the present note, only those parts (much the most important) of the majority and minority opinions are considered which discuss the matter of depreciation. There was also discussion (as well as dissent) as regards the fair rate or percentage of return, which should be allowed on the basic sum — the rate base. The majority held that a return of 7.44 per cent sought by the Company itself, was the least that could be deemed non-confiscatory; whereas Justices Brandeis and Holmes maintained that "a net return of 6.26 per cent would seem to be compensatory."

what exhaustion is to be provided for, and, in part, one of expressing the exhaustion in terms of money. Under the first head come such questions as whether the allowance is to cover only wear and tear, or wear and tear and also obsolescence; and whether it is to be measured by observation, or in some other way. Under the second head comes the question whether the exhaustion is to be expressed in terms of money on the basis of original cost, present reproduction cost, or probable replacement cost.

Questions under both heads had been considered by the Maryland Commission and the Maryland Court of Appeals, but only the second phase was discussed by the Supreme Court. The Maryland Commission, in its decision of the rate case, had adopted two principles: first, that the annual allowance for depreciation should be determined on the basis of original cost; and second, that the allowance should be sufficient to "provide a fund out of which retirements may be met as they occur, and also to create a reserve out of which ordinary obsolescence may be cared for."

The Court of Appeals of Maryland had approved the Commission's decision on the second point but had held that the allowance should be based on present value instead of original cost. The Commission had then recomputed the allowance on that basis, the result being an increase in the amount for the year under review from $883,544 to $1,658,650.

The Supreme Court, in concluding that the order of the Maryland Commission was confiscatory, deducted the larger sum of $1,658,650 from the earnings as the necessary provision for depreciation, thus in effect upholding the decision of the Maryland Court of Appeals on both points. It did not discuss the principle that a depreciation fund should provide for retirements and ordinary obsolescence, but its approval of this principle is clearly implied in, and necessary to, the conclusion reached.

In the majority opinion of the United States Supreme Court, the discussion of the depreciation question begins as follows: "The allowance for annual depreciation made by the Commission was based upon cost. The Court of Appeals

held that this was erroneous and that it should have been based upon present value. The Court's view of the matter was plainly right." But in spite of this clear-cut declaration, the effect of the decision on the question of the basis on which the exhaustion is to be expressed in terms of money is not free from doubt.

The standard sum with which the probable yield of a rate structure is to be compared in order to determine whether that structure is compensatory or confiscatory may be conveniently regarded as composed of three parts:

1. An amount to cover a fair return on capital investment.
2. An amount to cover exhaustion of property.
3. An amount to cover all expenses of or incidental to operation except those of making good exhaustion of property.

The first amount is, under a long series of decisions of the Supreme Court, in the nature of compensation based on value, not on cost. The third is merely a reimbursement of cost. The question remains whether the second amount should be determined like the first, as compensation based on value, or like the third, as a reimbursement of cost? If the latter, should the measure be the original cost of the property exhausted or the cost of making good the exhaustion?

The majority opinion decides categorically that the allowance for exhaustion must *not* be computed on the basis of original cost of the property exhausted. It seems to leave open, however, the question whether the allowance should be computed as a compensation based on value, or as a reimbursement of cost based on the actual or probable cost of replacing the property exhausted. For after upholding, in the language above cited, the use of present value in the case at bar the Court entered upon a discussion which leaves a doubt as to whether present value was adopted as the basis because it was present value or because it was the most practical approximation to probable cost of replacement.

After quoting the language of the Knoxville Water Company case to the effect that the utility "is entitled to see that

from earnings the value of the property invested is kept unimpaired, so that at the end of any given term of years the original investment remains as it was at the beginning,"[2] the Court adds: "This naturally calls for expenditures equal to the cost[3] of the worn-out equipment at the time of replacement; and this, for all practical purposes, means present value."

So far, the argument would appear to lead to the conclusion that in principle the allowance was to be based on actual or probable cost of replacement, and that for the purposes of the instant case, at least, present value was a reasonable guide to probable cost of replacement. But in the ensuing sentence the Court said: "It is the settled rule of this Court that the rate base is present value, and it would be wholly illogical to adopt a different rule for depreciation." Here, the Court seems to adopt present value upon its own merits and not as a guide to replacement cost.

Mr. Justice Stone, while concurring in a dissenting opinion of Mr. Justice Brandeis, added a vigorous attack of his own on the validity, either generally or in this particular case, of the assumption that replacement cost means, for all practical purposes, present value. His opinion concludes as follows:

> To say that the present price level is necessarily the true measure of future replacement cost is to substitute for a relevant fact which I should have thought ought to be established as are other facts, a rule of law which seems not to follow from Smyth *v.* Ames, and to be founded neither upon experience nor expert opinion and to be unworkable in practice. In the present case it can be applied only by disregarding evidence which would seem persuasively to establish the very fact to be ascertained.

Mr. Justice Stone did not discuss the argument that it would be illogical to adopt a rule for depreciation different from that adopted in determining the rate base. Mr. Justice Brandeis rejected the argument, on the ground that the depreciation charge is designed to distribute the total net expense of plant replacement over the period of use. One is

2. In passing it may be remarked that this sounds like an echo of the prudent investment theory.

3. Cost here evidently means cost of replacement, not original cost.

probably justified in concluding that the Court upheld the present value basis as being, for all practical purposes, equivalent to the cost of replacement, and that the argument by analogy from the method of determining the rate base was thrown in rather as a makeweight.

Whether a depreciation scheme for a regulated utility should be based on original cost or probable cost of replacement is, from a practical standpoint, a relatively minor consideration. But it is essential, and the point is important, that depreciation shall be recognized as a reimbursement of cost and treated accordingly. Certainly there is nothing in the decision to preclude the Court from so interpreting it in any future case, and the hope may be expressed that it will adopt this interpretation should the question arise, as it probably will.

Any depreciation scheme is necessarily based to a large extent on estimates and assumptions. In order that it may operate equitably, the original estimates and assumptions must be reasonable, the scheme must be continuously and consistently carried out from year to year, and proper provision must be made for the correction (without undue disturbance of the operating or financial situation) of estimates made in earlier years which in the light of later experience may prove to have been too high or too low. A further essential is that the theory of distribution of the cost of property over its useful life shall harmonize with the theory of valuation on which the rate base is to be determined. Finally it is highly desirable, if not essential, that the scheme shall be suitable for the current accounting of the utility, as well as applicable in the determination of rates.

All these requirements can be met so long as the allowance is treated as a reimbursement of costs, actual or prospective. If replacement cost is used rather than original cost, the basic problem is not changed, even tho a new assumption becomes necessary, and continuity and provision for adjustment become the more necessary. If, however, the view were to be accepted that for rate purposes the allowance for any year should be determined as compensation for the exhaustion of

life occurring within the year, measured by observation and expressed in money in terms of current reproduction prices, the whole computation being made without regard to the provision made in any other year or to the actual outlays for original purchase or replacement, then all the essentials above recited would be sacrificed and a method would be substituted which would be discontinuous, conjectural, and utterly unsuited for any other purpose, if not, indeed, for the purpose contemplated.

These considerations become even more important if the depreciation scheme aims to provide for obsolescence as well as mere physical exhaustion. In the Baltimore case, the Commission upon the original hearing stated its position as follows: "In making an allowance for depreciation the Commission will endeavor to provide a fund out of which retirements may be made as they occur, and also to create a reserve out of which ordinary obsolescence may be cared for." As already stated the Court of Appeals of Maryland upheld the Commission on this point.

Now, allowance for obsolescence is, to repeat, hardly practicable except under a scheme continuously and consistently applied, with provision for adjustment of estimates on the basis of experience. Accounting control and observation are both essential to the proper administration of such a depreciation plan. The courts, whose relation to the question is more remote and spasmodic, are apt to stress the importance of the second essential, observation; but commissions, whose relation is closer and more continuous, perhaps rezalize more fully the importance of accounting control.

With the modern development of accounting methods, book records, supplemented by such engineering reports as a competent accounting officer will naturally call for, will prove the most effective and reliable basis for computing and adjusting depreciation allowances. To illustrate the point from another field; book inventories, properly maintained (as they now are by most important corporations), have proved to be more reliable in the case of any large and complex business than inventories based on physical examination, count,

weight, and measurement. Much the same is true in the field of depreciation.

It has been mentioned above as an essential of a sound system of depreciation for regulated utilities that the methods and theories upon which the annual allowances are computed must harmonize with the methods and theories governing the determination of the rate base. It would seem inevitable that the courts must accept this principle when once it is clearly presented. If obsolescence is to be allowed for in determining the annual allowance, it must be allowed for in the fixing of the rate base — otherwise, the consumers will inevitably be subjected to a double charge. But the application of the principle obviously presents a problem of considerable difficulty.

In this problem, obsolescence may be considered in two parts: first, what may be called demonstrated obsolescence; second, potential obsolescence. At the earliest stage — say at the end of the first year of operation of a new plant — some reserve will have been set aside for obsolescence; but presumably there will be little if any demonstrable obsolescence to be provided for and the reserve will be applicable only to potential obsolescence. How is such potential obsolescence to be allowed for in the determination of the rate base?

If the rate base were being determined on the basis of original cost, it would be a simple and equitable solution to treat the excess of the reserve set aside for depreciation in the year, over and above the observed depreciation, as being a reserve for potential future obsolescence, and the desired consistency would be secured by deducting it, as well as the observed depreciation, from that cost, to arrive at the rate base.

Where the rate base is computed on present value, probably the only practical way of making the proposed adjustment would be by use of ratios. The cost value of the property subject to obsolescence and the reserve for obsolescence in respect thereof being known, the deduction for obsolescence from the reproduction value might be computed at the same percentage as the actual reserve for obsolescence might be found to bear to the cost value of the property subject to obsolescence.

In the case of the plant of a utility which had been operating for a long period of years, there would be both demonstrated or observable obsolescence, and potential obsolescence. It would certainly be desirable, and should be practicable, to express the demonstrable obsolescence in terms of money.

Let us suppose, for instance, the case of a unit which cost originally $1,000 but would now cost $1,200 to replace in kind. Assume that it has been in service ten years and that from the purely physical standpoint it would be good for another ten years' service. Suppose, however, that it is capable of being replaced with a unit of a new type which would cost $900, which would be estimated to have a life, allowing for physical exhaustion and ordinary obsolescence, of fifteen years, and would effect an economy in operation of $36.00 per annum. Let it be assumed that the fair return on investment in the utility is 7 per cent, and that depreciation allowances are being computed on a sinking-fund basis, with interest at 4 per cent.

Upon the foregoing suppositions, would it not be fair to hold that the value of the existing unit, depreciated for obsolescence, is determined by consideration of the cost of the new and superior type? If so, it seems clear that allowance for demonstrated obsolescence (even without taking into account further potential obsolescence) brings a result that is very close to valuation on the basis of the current cost of the most effective unit that could be substituted for the existing unit, rather than upon the cost of reproducing the unit actually existing.[4]

4. The calculation in detail might be as follows:
The sinking fund required to write off $900 in fifteen years at 4 per cent would be $44.95; the annual return on $900 at 7 per cent would be $63.00 — together, $107.95. Deducting $36.00 from this amount for the greater economy of the newer unit leaves a balance of $71.95, which would seem to represent fairly the annual value in service of the existing unit. The corresponding capital value of the unit on the basis of ten years' unexpired life, allowing a 7 per cent return and a 4 per cent sinking fund, would be $469.37.

The courts in the past have been reluctant to go into this

question. In the Indianapolis Water Company case (272 U. S. 400) the Supreme Court said:

> There is to be ascertained the value of the plant used to give the service and not the estimated cost of a different plant. Save under exceptional circumstances, the Court is not required to enter upon a comparison of the merits of different systems. Such an inquiry would lead to collateral issues and investigations having only remote bearing on the fact to be found, viz., the value of the property devoted to the service of the public.

But with all respect, it would seem that the Court here begged the question. Why should the *value* of the plant actually used be measured by the *cost* of reproducing it, if a more efficient type of plant could be constructed at a materially lower cost? Even if the new value of an existing plant may properly be computed on the basis of reproduction cost, certainly its value as depreciated for use or obsolescence cannot be determined without consideration of the question of its relative efficiency and economy in construction and operation as compared with the most efficient and economical substitute that could be created.

This line of thought suggests that the implicit approval by the Supreme Court of the recognition by the Commission of obsolescence as an element of depreciation in rate cases may be quite as important as its explicit rejection of original cost as the basis of the calculation of the allowance. Indeed it may be that further consideration of the depreciation question may eventually shake the foundations of the current theory of present value. Even if it be granted that, in principle, present value is a more appropriate basis for calculation of the fair return than original cost, it is obvious that the current theory of computation of present value is highly artificial and the results often lacking in reality.

The validity of the conclusions reached under the current theory depends on the validity of the underlying assumptions which must be made if a computation is to be possible. It is necessary to make assumptions as to the conditions immediately prior to the theoretical reproduction, which, in the case of a really important property, such as a large railroad

system, are highly speculative and unreal. Again, as already noted, in order to keep the inquiry within bounds the courts have felt compelled to indulge the presumption, frequently unwarranted, that the existing plant would be worth reproducing if it did not exist. For instance, the value of a building and the site on which it stands is assumed to be the aggregate of the value of the land and of the cost of reproduction, less depreciation, of the building; tho the land may be taken at its value for an entirely different purpose, and the true aggregate value of the property may be the value of the land, less the probable cost of removing the building.

The resulting valuation is thus a conventional, not a real figure. This may not be a fatal objection, but at least conventions should be applied with reasonable consistency. If it be accepted as a convention that properties properly maintained do not depreciate, then it should be accepted as a convention that earnings are determinable without any allowance for depreciation. If a convention closer to reality is to be adopted, that properties depreciate through age and obsolescence, even tho well maintained, and that an allowance for such depreciation must be made in determining earnings, then allowance for depreciation from age and obsolescence, and not merely for observed depreciation, should be made in fixing present value.

How far afield and to what results the consistent application of the doctrines and conventions now current would lead is a question of vital importance to public utilities and to the public. At the least, it would seriously modify existing practice in rate regulation; it might result in the breakdown of the whole present system. Discontent with that system is rife, and much of the discontent well-founded. It is perhaps a question whether regulation on the basis of fair return is really practicable or effective. Constitutional limitations may prevent the substitution of other methods which have been suggested. But, if this be so, the result may be a drift toward something entirely different, public ownership.

Taking the long view, no problem perhaps possesses more importance for the utilities than that of presenting some con-

structive plan for securing justice to the consumers as well as reasonable opportunities for profit-making for themselves. At the moment, however, the utility managers seem, in general, content to do nothing, and to rely upon the decisions of the Supreme Court, and upon the fog with which the whole question is shrouded as a result of those decisions, for the maintenance of a situation which for the time is financially satisfactory. They are not likely to change their attitude until they are convinced that the protection on which they rely is not so complete nor so well assured as they have thought it to be. There is danger to them in a fuller recognition by the Court of the limited significance of cost of reproduction of existing plant and of the essential importance of coördination between the different parts of the problem of regulation, and more particularly, between the charges against earnings for depreciation and the deduction from the rate base on account thereof.

The Supreme Court is not committed to any rule of law which assures to cost of reproduction the importance which in practice has been given to it in some recent decisions. If the Court should accept — as in the end it may be compelled to do — the logic of cost of production of the most efficient substitute (less depreciation) as the basis of valuation, while the results would still be uncertain, the uncertainty would be fraught with grave danger to the utilities. They would be wise to recognize this danger, to admit the unsatisfactory state of regulation, and to devote their experience and their knowledge of the problem to finding a solution that would be just to the public as well as fair to themselves.

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COMMERCIAL POLICY AND ITS SCIENTIFIC METHOD: A REPLY

THE generally unfavorable review of my Aussenhandel und Aussenhandelspolitik, published in the November issue of this journal, calls for a reply on two counts. For one thing, Professor Graham's remarks are likely to give a false impression as to the position of German scholarship on the question of commercial policy, especially since the book appeared as a part of the widely used Grundriss der Sozialökonomik; they raise, moreover, certain methodological questions which it is in the interest of science to clarify by means of discussion. It is not my purpose, however, to take up each of his criticisms separately, altho none of them seems to me justified.

I shall begin with the attitude of my book with respect to commercial policy. To my considerable astonishment I learned from the review that I am a supporter of protection! The contrary conclusion is to be drawn from the work! As a scholar, of course, one's task is not to advocate a particular policy, but to examine the arguments pro and con from a logical point of view. Professor Graham has apparently failed to see that I cite the arguments, even the opposing ones, objectively and as effectively as possible, without identifying myself with them in any way. My preference is rather for qualified free trade — no different from that of Smith and Ricardo. For Professor Graham's general appeal to the "classical writers" is somewhat open to question. What writers? I made explicit reference to Smith's well-known four exceptions. I do not adopt them all, myself. Say is a supporter of protection in decisive passages in his work. Need I refer to Malthus' The Grounds of an Opinion or to chapter 12 (vol. ii) of his Essay, which contain the essentials of the whole modern theory of protection? That the arguments of protectionists must be taken seriously goes without

saying for a man of science, all the more since the whole world, under the leadership of the United States, has become predominantly protectionist-minded during the last sixty years.

The following citations suffice to show the generally free-trade spirit of the book.

(1) On page 101 the chapter concludes:

> In the classical free-trade argument considerations of production policy and consumption policy are lined up together. . . . They constitute a phalanx, the lasting importance of whose power and the logical soundness of whose proof is not to be denied.

(2) Page 102 states:

> In the whole argument for free trade there are great truths which must not be lost sight of — the fundamental idea of a division of labor; the selective power of international competition, of technical progress; the argument as to consumer interests and export needs, and of the inner relation and reciprocal action of tariffs upon one another. They must always be taken into account in a discussion of international problems.

Then *new* arguments in support of free trade are expressly introduced, which in this form are to be found together scarcely anywhere else: the argument of the increased sale and decreased cost resulting from a broadening of the market; the wage argument of the converse theory; and the crises argument. (Pages 101 ff.)

(3) On the other hand, almost all the arguments for protection were called logically inadequate. Thus the arguments in support of the self-sufficient tariff system are regarded as "faulty and inconsistent."

> Its scope remains limited because no country can follow such a course without injuring itself in relation to other countries in the long run. The system remains inappropriate because it is unable to attain what it is aiming at. If consistently carried out, it would surely lead to a permanent burdening of the nation through the preservation of unadapted industries and hamper the development of the productive forces, by supporting all attempts at [industrialization], even those inherently hopeless. [Page 115.]

The young industry argument (Erziehungsargument) is likewise rejected.

Ostensibly it sketches a large problem in unequivocal terms, and settles on a definite choice; but in not a few critical cases it proves a failure. It fits, fundamentally, only for a particular historical stage of individual countries: without, however, being able to define the period of its validity, and without offering a general solution. [Page 121.]

The same with the principle of social equalization, which has supporters in the United States above all. "However plausible it may sound, it refutes itself." (Page 125.)

Even in the case of dumping (pages 126 ff.) protection does not seem to me justified. In the long run the dumping country is itself badly injured; according to the theory of equilibrium the value of the imports must be less, as a result of exporting at lower prices, than the value of the domestic goods given in exchange for them.

(4) In only one case do I consider a protective tariff possible and effective, namely, where investments of capital are *temporarily* imperilled by foreign competition. In the absence of protection, it is to be expected that after a certain time the interrupted production must be built up anew, because foreign countries cannot in the long run supply the demand at lower prices. That is the case wherever the costs of building up the industry again are greater than the expense of maintaining existing industries. It would be possible, for example, to abandon mines of low yield when cheap supplies were to be had from abroad. But if we foresee that as a result of the law of diminishing returns in the foreign mines the domestic mines may become profitable again, a cessation of operations would be economically harmful: the restoration of the whole equipment would require excessive costs. The same may be true of agriculture in old industrial countries. For the assumption of free mobility of capital is nowhere justified when great amounts of capital are permanently invested, whether in agriculture or in manufacturing. Since frequent shifting of this capital causes a loss, maintenance by means of governmental measures, in some cases even by tariffs, would seem to be economically imposed.

This is Adam Smith's fourth argument, somewhat differently formulated, and modernized. It was also discussed

by Ricardo himself, however, who declared with reference to agriculture:

> To establish measures which should at once drive capital from the land would, under the present circumstances of the country, be rash and hazardous, and therefore I should propose that the duty of 20 shillings should every year be reduced 1s. until it reached 10s. [On Protection to Agriculture, "Conclusion."]

Of manufactures, concerning which the same may be said in analogous cases, he was not yet able to speak in his day. The idea of decreasing tariffs, which I repeatedly mention, was supported by Ricardo as well as Say.

(5) The "most favored" system was expressly defended by me, because it tends to make the lowest actual tariff rates general. The objections to it were dismissed quite in the spirit of Viner and Taussig. (Pages 189 ff.)

(6) The idea of a customs union, which is much discussed in Europe, is opposed on the ground that it would lead to a raising of tariffs and tend to make them permanent. The highest rate of protection needed by any member would have to be applied to all.

> The goal of a greater market, the extension of the radius of production, could be attained just as surely and in a much better way by means of a free-trade policy. In comparison with this a customs union is a very incomplete means and only a kind of temporary solution. [Page 120.]

(7) In considering industrial tariffs it was pointed out that they cause a rise in prices and may easily lead to a wrong use of capital. In this way overproduction is brought about in some branches of industry and the elimination of weak concerns is retarded. (Pages 249 ff.)

(8) It is essential — and in this my book is distinguished from those concerned only with commercial policy — that the other *administrative measures* should also be weighed with respect to their influence upon international trade. These protectionist measures now play a very great part in every country. It therefore had to be shown that they are a decidedly two-edged weapon. For we should never

forget the principal thesis, . . . that is, the market character and two-sidedness of all economic relations. *In international trade in commodities you cannot sell more than you are willing to buy yourself or to accept services for*, and vice versa. [Page 70.]

(9) A special chapter is devoted to the possibilities and limits of governmental measures. Here, too, manipulation of the trade balance is expressly rejected. It

cannot be applied consistently without disturbing international relations generally and injuring the export trade as well. The attempt will therefore bring harm upon the country's own economic life. . . . The manipulation of the trade balance by means of restrictions on imports must therefore take place at the expense of exports or of borrowing from abroad. Not even the restriction of so-called luxury imports promises any success in this respect. [Page 282.]

As for the question of method, I am the more inclined to go into that since I happen to have undertaken extensive investigations into the logic of the social sciences.[1] It is an error to call the method I have used historical. On the contrary, the deductive-analytical method of isolating abstraction is used where ever it is a question of clarifying functional relations. The repeated references to Ricardo show that I continue here along the lines laid down by this great predecessor, without following him, to be sure, on every point. A few examples will sufficeto show that my critic is mistaken.

(1) The analysis of the shifting of import duties is a continuation of the ideas of Marshall and Pigou, especially as regards elasticity of supply and demand; it is naturally purely deductive and supplies the foundation for the whole third book dealing with "economic significance."

(2) The effect of particular kinds of tariffs could also be explained only by isolating the factors. The same is true of the analysis of the relations between world prices and national prices. It is true that I did not offer any special proof that rich countries have a higher price-level than poor countries, but took this for granted as obvious. It is not difficult to

1. "Naturgesteze und soziale Gesetze, Logische Untersuchungen," Arch. f. Sozialwissenschaft und Sozialpolitik (1911-12), vols. xxxi, xxxii.

prove, however. The greater wealth of a people shows itself in the absolutely higher *per capita* income, and also in the larger numbers in the upper income-groups. Consequently the purchasing power of the latter will have greater weight than in the countries where these groups are smaller. Greater demand, however, *ceteris paribus*, must cause higher prices. The fact that two price-levels are not comparable *in detail* by no means leads to the conclusion that one may not be absolutely higher than the other. In the same way, even tho two bodies have different shapes, one may be larger than the other. If a second edition is called for, I shall develop this line of reasoning more fully, with special reference to Professor Graham's criticism.

(3) Since demand is a function of prices, the use of "weights" in determining the price-level seems logically necessary. I pointed out that, even when some individual prices rise, there need be no rise of prices in general. It is more probable that the purchasing power of certain classes for some kinds of things will be reduced as a result, and that the weights will have to be readjusted. I pointed out this very fact as a possible and, indeed, unfavorable effect of national tariffs!

(4) Contrary to Professor Graham's assertion, the idea of equilibrium stands in the foreground of the whole work; it is based upon the market organization of our economic system. "Foreign commerce in the sense of international economic intercourse postulates two-sided transactions. Every sale by one party necessarily liquidates a counter transaction." (Page 5.) The equalization of international payments rests upon the formula

$$F\ (k+p+d+w) = V\ (k+p+d+w)$$

which dominates the whole book. (F = credits, V = debits, k = services of capital, p = tourist expenditures and migration, d = services, w = commodity trade.)

(5) The "economic coefficients" are often mentioned, i. e. the relation between net yield and expenditure, which can be

expressed by a mathematical formula, even tho the formula $\frac{n}{a}$ or $\frac{n}{k}$ is not expressly named (n = net yield, a = labor, k = capital). Chapter 6, which is devoted to the idea of economic capacity, presents a first attempt to grasp the relation between foreign markets and domestic markets in generalized form, and arrives at certain linear equations as a first approximation. Is all that no more than a historical conception of complex forms?

In all this no procedure but that of isolating abstraction was possible. In other parts of the book, where political measures have to be described, another method was the obvious one to use: again, not that of the Historical School but one which everywhere tries to show analytically a functional relation between measures and results.

(6) On one critical point, it is true, my view differs fundamentally from Professor Graham's. There are no categorical (absolute) laws of knowledge, as he assumes, but only hypothetical ones, i. e. every functional relation between several magnitudes is bound up with particular presuppositions, which are assumed to be correct. These presuppositions themselves, however, are not fixed once and for all, but are variable and must be examined individually. If they change, then the result, as in all mathematical propositions, will also be different. This is characteristic both of natural laws and of social laws. The proof of this is developed in my "logical inquiries." Ricardo, above all, starts tacitly with certain presuppositions and conditions, which are nowadays usually referred to as "data." These presuppositions are very definite and often only historically conditioned, but by no means of general validity. The applicability of the law of comparative costs is therefore, as I tried to show, very restricted. If we wish to arrive at a *general* law, we must vary the presuppositions. In that case, however, the final conclusion may be different, simply because there is only *one* logic. If we assume that the quantity of labor is qualitatively different in the different countries, we reach different results. It is interesting to vary Ricardo's presuppositions in this respect, as Manoilesco

in his Théorie du Protectionisme (1929) has recently tried to do. It would be worth while to enter on a more detailed consideration of this point.

(7) If, however, we refuse to accept Ricardo's law of cost at all in its previous formulation, as the marginal utility school must, the theory of comparative cost loses its basis. I have tried — and moreover, in accord with Mill's example — to give prominence to the idea of comparative demand. It is to be found in a number of passages in Mill. For example, Book III, chapter 18, section 2:

> It may be considered as established, that when two countries trade together in two commodities, the exchange value of these commodities relatively to each other will adjust itself to *the inclinations and circumstances of the consumers on both sides, in such manner that the quantities required by each country . . . shall be exactly sufficient to pay for one another.*

Or ibid., section 8:

> The only general law, then, which can be laid down, is this. The values at which a country exchanges its produce with foreign countries depend on two things: first, on *the amount and extensibility of their demand for its commodities compared with its demand for theirs;* and secondly, on the capital which it has to spare, from the production of domestic commodities for its own consumption. [The italics in these passages are mine.]

For price-formation and therefore exchange are also dependent in international trade upon effective demand. It was with this very thesis in mind that I placed the chapter dealing with the moving forces in international economic intercourse first, because otherwise the idea of "comparative demand" would not have been understandable; it is an integral part of the fundamental concept of my work.

I believe that, when my esteemed critic thinks through the relation between presuppositions and logical conclusion, he will subscribe to my view. For after all the proposition that $2+2 = 4$ is only a definitory one, i. e., in the language of logic, an "analytical judgment," from which nothing can be proved.

I am in agreement with Professor Graham in thinking

highly of the classical writers. But Jean Baptiste Say, himself one of them, already, said in his Cours d'Economie Politique (3. éd., 1852, p. 41): "Comme si, en supposant qu'ils (les auteurs) fûssent au niveau des connaissances actuelles, les lumières n'étaient pas essentiellement progressives; comme si l'expérience de demain ne devait rien ajouter à l'expérience d'aujourdhui, et comme si la plus grande sagacité pouvait prévoir toutes les conjonctures à venir, et toutes les applications possibles." What makes the study of the classical writers so fruitful, it seems to me, is the fact that they are capable of being extended and renewed.

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A CRITICISM ON BYE'S THEORY OF PUBLIC UTILITY RATE-DETERMINATION

PROFESSOR RAYMOND T. BYE, in an article on "Composite Demand and Joint Supply in Relation to Public Utility Rates" in the November (1929) issue of the Quarterly Journal of Economics, sets forth what might be called a theory of public utility rate-determination. His fundamental thesis is that public utility rates "should conform closely to competitive prices" because "rate regulation is intended largely to do for the monopolistic public utilities what competition is expected to do for competitive, unregulated industries." Having set forth this thesis, he proceeds to apply the theory of normal value to the determination of public utility rates, taking up first the case of composite demand, then that of joint supply.

It is not here proposed to discuss the validity of Professor Bye's contention that competitive standards can be readily applied to public utilities. This would involve perplexing problems of the proper rate base, of the possibility of applying the normal value theory to industries which cannot be

easily abandoned, and so on — matters as difficult as they are familiar. The present note will be confined to the point that there is no warrant for Professor Bye's conclusion: ". . . in a simple case of composite demand, no discrimination in the distribution of the overhead burden among different classes of consumers is justified" (page 53).

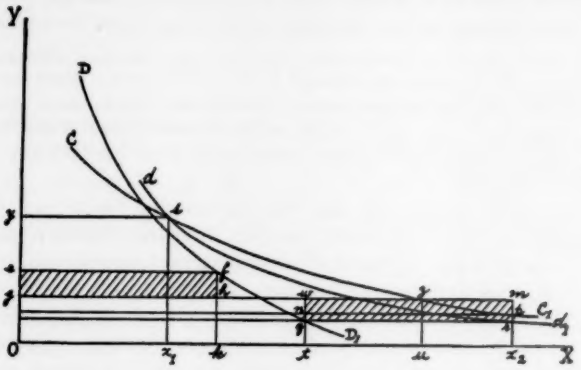
Behind this conclusion, and in fact, behind the whole of Professor Bye's thesis, lies an assumption that public utility plants are working at or near the least cost point, i. e. that there is no large surplus capacity, actual or potential. Professor Bye recognizes this, and justifies his position by saying:

> It cannot be maintained that this [additional] business will make for a more effective utilization of plant capacity, unless the plant is already too large for its market — a condition, which, under good management, ought not to exist, and for which no class of consumers ought to be penalized by being made to pay an excessive burden of overhead [page 52].

It is very well to say that surplus capacity "ought not to exist"; but if it does exist, that fact surely constitutes a part of the problem. In many railroads, the existence of such surplus capacity is an admitted fact. In other public utility undertakings, even a casual observation will reveal that many plants are being operated at a point considerably above the least cost point. True, most companies are operating a part or parts of their plant at capacity. Many gas companies, for instance, are using their reservoir tanks to full or nearly full capacity; but not their pipes. Under such conditions, any substantial increase in the output would necessitate a capital expenditure on those parts only of the plant which are now being used to capacity. This expenditure would, for a time, increase the per unit cost, but, as output increases, the per unit cost would decrease until such time as the plant is working at the least cost point. The accompanying diagram is an illustration of such cases of potential or actual surplus capacity in industries subject to composite demand.

Along the Y axis is measured the per unit share of overhead expenses, while the quantity of the commodity or service in question is measured along the X axis. The curve CC_1 rep-

resents the average per unit share of overhead cost. The curve DD_1 represents what Professor Bye calls the net demand schedule of one class of consumers, and dd_1 represents likewise the net demand of a second class of consumers. This curve dd_1 is so drawn that its abscissae are measured not from OY but from DD_1. If we follow Professor Bye's dictum that no discrimination is justified in cases of simple composite demand, the per unit share of the overhead expenses would be Oy. Assume now that in order to take full advan-



tage of the surplus capacity, we lower the price to such an extent that each unit contributes toward overhead cost an amount represented by x_2r. At this price, however, there is loss of rp for every unit sold; the total loss from sales to the second class of consumers alone would be represented by the narrow rectangle $nqrp$. Obviously, all the quantity Ox_2 cannot be sold at this price. The price to the first class of consumers is therefore advanced to a point high enough to cover not only all the cost attributable to sales made to this class, but also the loss incurred from sales to the second class of consumers at such a price that the overhead expense actually borne by each unit is only x_2r. Let this enhanced price to the first class be such that the per unit share of overhead cost borne by this class is kf. At this price, the quantity sold to

the first class would be *Ok*. The total quantity sold would now be, not Ox_2, but *Ok* plus tx_2, or Ox_2 minus *kt*. The total production would accordingly decrease, and the average per unit share of overhead expenses would increase. To ascertain this increase, locate a point *u* on the *X* axis such that the distance ux_2 (*u* being nearer *O* than x_2) would equal the distance *kt*. Then, since *kt* equals ux_2, Ox_2 minus *kt* equals Ox_2 minus ux_2. But Ox_2 minus *kt* equals the total production; therefore Ox_2 minus ux_2 also equals the total production. But, since Ox_2 minus ux_2 equals *Ou*, *Ou* must also equal total production. Then *uz* is the average per unit share of overhead expenses. The loss incurred from the sale of tx_2 quantity at such a price that each unit contributes only x_2r toward overhead cost, is now represented by the shaded rectangle *wqrm*. This loss must obviously be made up from sales to the first class of consumers. The average per unit share of overhead costs being *uz*, or *kh*, and the per unit share of overhead costs borne by the first class of consumers being *kf*, the shaded area *eghf* represents the net gain over and above all expenses. The shaded rectangle *eghf* must therefore equal in area the other shaded rectangle *wqrm* if the loss from sales to the second class of consumers is to be made up by the gain from the first class. (In practice, these points must be ascertained through the trial and error method.) There is here a discrimination in favor of the second class, but it is evident that this discrimination has benefited not only the second class but also the first class. The consumers of the first class have formerly paid the per unit share of overhead cost represented by *Oy;* now they pay only *Oe*. The extension of the use of the plant to, or near, the least cost point is thus justified: the whole plant earns a fair return, and both classes of consumers now pay less than before.

In our illustration, there is an assumption of a large surplus capacity, actual or potential. That such surplus capacity exists in many of our railroads is an admitted fact. It also exists in many of our utility companies other than railroads. The assumptions made by Professor Bye are plainly unwarranted when he states:

It may be true that the increased business occasioned by supplying gas for industrial fuel at low prices will make possible the maintenance of a larger plant, thereby reducing the average unit cost of gas manufacture; but notwithstanding this, other consumers will have to pay a higher price for gas than would otherwise be necessary, *for the reduction in average costs of manufacture can never be as great as the loss occasioned by selling to the new group at less than average costs* — a loss which must be made good by charging higher prices to domestic consumers. . . . The conclusion, then, is that *in a simple case of composite demand, no discrimination in the distribution of overhead burden among different classes of consumers is justified.* [Pages 52–53. Italics are present writer's.]

Professor Bye's conclusion, in fact, was refuted several decades ago when Hadley observed in his Railroad Transportation:

If our railroads made it a rule to carry nothing at less than the average cost of doing the whole business, they would give up nearly all the coal trade and a great deal of the grain trade. It would give us dear food and dear fuel, and would injure both the railroads and the districts which they serve. [Page 114.]

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